Fair Value Accounting and Informational Efficiency: A Look at the Confirmatory Role of Financial Reports

by

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Author's Declaration

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

I understand that my thesis may be made electronically available to the public.

Abstract

Prior analytical research suggests that independently verified financial reports can enhance informational efficiency by serving a confirmatory role, where they discipline managers' unverified, but more timely, voluntary disclosures. I study the confirmatory role of financial reports by examining how fair value accounting affects two aspects of informational efficiency: the credibility of voluntary disclosures and the timeliness of price discovery. I argue that greater measurement uncertainty associated with fair values can make the accounting numbers less verifiable and potentially less reliable. This, in turn, can hinder the extent to which financial reports can serve a confirmatory role. Thus, I hypothesize that fair value accounting can reduce the credibility of voluntary disclosures and the timeliness of price discovery.

To examine these hypotheses, I exploit SFAS 133 (FASB 1998), which increases fair value accounting exposure for derivative users by mandating all derivatives to be reported at fair value. I compare the credibility of voluntary disclosures and the timeliness of price discovery of derivative users to those of derivative non-users, pre- versus post-SFAS 133, using a difference-in-differences research design. I identify derivative users using a combination of an engine-based keyword search and manual tracing to the 10-k filings on the SEC's EDGAR database.

Using management forecasts as a key voluntary disclosure, I find results suggesting that an increase in exposure to fair value accounting impairs the credibility of good news management forecasts, but not of bad news forecasts. A potential explanation for this asymmetric result is that bad news from management is inherently more credible and, thus, less susceptible to credibility concerns. In contrast, I find results suggesting that fair value accounting does not impede timely price discovery, but rather, can enhance timely price discovery in negative return periods. I also identify potential alternative explanations for these results.

In examining the impact of fair value accounting on the timeliness of price discovery, I find that the firm-level intraperiod timeliness metric, used in prior literature, has some limitations. Specifically, large return reversals during the period can lead to values that cannot be clearly interpreted as the timeliness of price discovery. I create a proxy to capture the extent of return reversals and find that the portfolio-level intraperiod timeliness metric mitigates such issues through averaging firm-level returns. In particular, using simulation analysis, I explore portfolio sizes that will mitigate these issues and use this to inform my analysis.

These results suggest that fair value accounting can have unintended adverse consequences for informational efficiency, by weakening the credibility of managers' voluntary disclosures. My findings are relevant to standard setters and regulators, given a continual transition towards greater fair value accounting. This thesis highlights the importance of considering the *system* of public financial reporting and disclosure, where the financial report is only one of many sources of information, when assessing the impact of accounting.

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Dedication

I dedicate this thesis to my husband and best friend, Steve, my dearest boys, Joseph and Caleb and my loving parents, Yoong Koo and Sae Jin.

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List of Abbreviations

AA	Arthur Anderson
ASC	Accounting Standards Codification
BIS	Bank for International Settlements
CEM	Coarsened Exact Matching
CIG	Company Issued Guidance
CRSP	Centre for Research in Security Prices
DiD	Difference-in-Differences
EITF	Emerging Issues Task Force
EPS	Earnings Per Share
FASB	Financial Accounting Standards Board
FF12	Fama French 12 Industry Classifications
IBES	Institutional Brokers Estimate System
IPT	Intraperiod Timeliness
MF	Management Forecast
MFRC	Management Forecast Response Coefficient
MTM	Mark-to-Market
OCI	Other Comprehensive Income
OTC	Over-the-Counter
PSM	Propensity Score Matching
Reg FD	Regulation Fair Disclosure
SFAC	Statement of Financial Accounting Concepts
SFAS	Statement of Financial Accounting Standards

Quote

"Financial reporting conveys an important economic role by accurately and independently counting actual outcomes, and hence confirming prior information about expected outcomes. In particular, if managers believe actual outcomes are more likely to be reported accurately and independently, they are less likely to disclose misleading information about their expectations." (Ball 2006, p.14)

Chapter 1

Introduction

Prior literature (e.g., Healy and Palepu 2001; Kothari, Ramanna and Skinner 2010) suggests that the objective of a public financial reporting and disclosure system is to facilitate efficient allocation of resources in the economy. Challenges arise due to information asymmetry and agency problems, which hinder the efficient allocation of resources in capital markets. Accordingly, Healy and Palepu (2001, p.407) state, "[d]isclosure and the institutions created to facilitate credible disclosure between managers and investors play an important role in mitigating these problems." These arguments envision a clear objective for financial reporting - alleviating information problems to facilitate efficient capital allocation in the economy. What is less clear is *how* financial reporting should achieve this objective. Specifically, what *primary* role should financial reporting serve in the system of public financial reporting and disclosures to best achieve this outcome? I emphasize the term 'primary' as financial reporting serves multiple roles and these roles are not necessarily mutually exclusive, but the desirability of accounting attributes hinges on our beliefs about which of these roles is more important.

Two principal views on the primary role of financial reports in a public financial reporting setting are evident from prior literature. Under one view, the financial reports' primary role is to provide stakeholders with new valuation information. While financial reports can certainly provide new information to investors, prior capital market research indicates that the amount of new information in periodic financial statements is rather small (see Lev 1989 for a survey of returns/earnings studies). Ball and Brown (1968, p.176) suggest that the reason for the lack of new information content reflected in earnings is because "the annual income report does not rate highly as a timely medium." Accordingly, Ball and Shivakumar (2008) and Ball (2013) propose that the primary value of these reports is, perhaps, outside of the valuation role. In support of this view, Bauer, O'Brien and Saeed (2014) advocate that the primary value of audited financial reports is in their ability to confirm and discipline managers' more timely voluntary disclosures. Lee (2014) also supports this view in asserting that the financial reports' ability to serve as an ex post settling-up mechanism is critical to the credibility of earnings forecasts.

This study investigates financial reports' ability to exert an accountability discipline on managers' voluntary disclosures by examining the effect of fair value accounting on a firm's information environment. The transition towards greater fair value accounting in the last four decades fuels an ongoing debate regarding the trade-off between the relevance and reliability or verifiability of fair values (see Kothari et al. 2010). At a more conceptual level, I posit that this debate reflects differential views on what role accounting should primarily serve. Although relevance and reliability are not mutually exclusive, relevance is more desirable when one views financial reports as primary sources of information (hereafter, valuation role) and reliability when one views audited financial reports as control mechanisms for the integrity of management's unaudited voluntary disclosures (hereafter, confirmatory role). Ball (2001) argues that financial reports that reliably report actual outcomes increase informational efficiency by enhancing the quality of managers' voluntary disclosures. Using this confirmatory perspective to formulate a link between financial reports and voluntary disclosures, I evaluate the impact of fair value accounting on voluntary disclosure credibility and the timeliness of price discovery. In particular, I highlight potential unintended consequences of fair value accounting, when considering the system of financial reporting and disclosures.

A maintained assumption in the literature on the confirmatory role is that the accounting numbers should be verifiable or reliable for financial reports to serve a confirmatory role (Ball 2001, 2006). Fair value accounting generally involves greater measurement uncertainty than historical cost accounting because fair values, unlike historical costs, are not always readily observable. Greater measurement uncertainty provides managers with greater discretion to report opportunistically. This, in turn, hinders the financial reports' ability to serve a confirmatory role (hereafter, confirmability) by reducing the accuracy with which financial reports can confirm the earlier voluntary disclosures. Thus, I predict that greater financial report exposure to fair value accounting reduces the credibility of voluntary forward-looking disclosures (H1).

To examine the impact of exposure to fair value accounting on voluntary disclosure credibility, I exploit SFAS 133 (FASB 1998), which increases fair value accounting exposure for derivative users by mandating all derivatives to be reported at fair value. I focus on SFAS 133 because the vast majority of derivative instruments are not traded on an exchange and, thus, their fair values often involve measurement uncertainty. Furthermore, the bi-directional nature of derivative fair value changes can increase or decrease earnings, providing management greater discretion to opportunistically misreport accounting numbers, relative to uni-directional changes such as goodwill impairment. I focus on management forecasts and examine the management forecast response coefficient (MFRC) as a proxy for the credibility of voluntary disclosures.

To test my hypothesis, I obtain a sample of annual earnings per share management forecasts for non-financial firms for the fiscal years ending June 1998 to May 2000 (pre-period) and June 2001 to May 2003 (post-period). I exclude financial firms because these firms often hold derivatives for trading or speculative purposes, which were already reported at fair value prior to SFAS 133. To identify derivative users, I use a combination of an engine-based keyword search and manual tracing to the 10-k filings on the SEC's EDGAR database. I use a matched sample to alleviate concerns that potential confounds, related to operational uncertainty or the richness of the firm's information environment, affect the MFRC differentially between derivative users and non-users.

Comparing changes in the MFRC of derivative users to that of derivative non-users, around SFAS 133, I initially do not find support for my hypothesis that an increase in exposure to fair value accounting reduces the credibility of management forecasts. However, I find that a substantial portion of my sample changes their decision to use/not use derivatives between the pre- and the post-period, leading to non-constant control and treatment groups that can confound results. Once I restrict the sample to treatment (control) firms that continue to use (not use) derivatives throughout the sample period (hereafter, constant derivative sample), I find some evidence supporting H1. Specifically, derivative users exhibit a more negative change in MFRC from the pre- to the post-SFAS 133 period than derivative non-users, suggesting that an increase in exposure to fair value accounting decreases management forecast credibility, when the forecast conveys good news. However, I find null results for bad news management forecasts. A potential explanation for the asymmetric result is that bad news from management is inherently more credible and, thus, less susceptible to credibility concerns as documented in prior literature (e.g., Jennings 1987; Skinner 1994; Williams 1996). However, these results may not generalize to firms that opt out of or into using derivatives in the post-period.

These findings are robust to including firm fixed effects, using an alternative matched sample, and using alternative specifications of the regression model. They are also robust to using three-day cumulative abnormal returns instead of two-day cumulative abnormal returns. I find stronger results supporting H1, relative to the primary results, once I exclude forecasts that are issued concurrently with earnings announcements. However, I find weaker evidence in support of H1 using the earliest management forecast for each firm-year, relative to using the latest forecast. Finally, I find that the findings are sensitive to whether or not I control for loss forecasts, as these observations are highly influential. When I do not control for these influential loss forecasts, I find null results for good news forecasts. However, if I include an indicator for loss forecasts or exclude loss forecasts, which comprise less than 2% of the constant derivative sample, results are similar to or stronger than the primary findings. Overall, these results suggest that fair value accounting can inadvertently impair the integrity of managers' voluntary disclosures, when the forecasts convey good news, but these results may not generalize to loss forecasts.

In my second hypothesis, I consider the impact of fair value accounting on the timeliness of price discovery. The prior literature (e.g., Rogers and Stocken 2005; Ng, Tuna and Verdi 2013) finds that investors discount their reaction to news that they perceive to be less credible. Further, Lennox and Park (2006) find that managers are less likely to issue management forecasts when the expected reaction to each unit of news in the forecast is lower. Collectively, these studies suggest that less credible voluntary disclosures reduce the timeliness of price discovery because investors will discount their reactions to these disclosures, which, in turn, lowers managers' propensity to issue voluntary disclosures. Hence, building on my prior argument that fair value accounting dampens the financial reports' confirmability, and, thereby, reduces voluntary disclosure credibility, I hypothesize that greater exposure to fair value accounting reduces the timeliness of price discovery.

Similar to H1, I use SFAS 133 as a shock to the exposure to fair value accounting for derivative users and compare the timeliness of price discovery between derivative users and non-users, before and after the enactment of SFAS 133. I use a portfolio-level intraperiod timeliness metric, *IPT*, that captures the speed with which information is reflected in prices. I test the difference-in-differences in *IPT* (*DiD_IPT*) using a permutation test, where I compare the observed *DiD_IPT* to a null distribution of *DiD_IPT,* created under the assumption that the order of the monthly returns does not matter.

The intraperiod timeliness (IPT) metric, which has been used both at the portfolio level and at the firm level in prior literature has some limitations.[1] *IPT* is a function of both the timing of news arrival and the speed with which this news is communicated and incorporated into stock prices. Thus, to interpret *IPT* as the timeliness of price discovery, it is necessary to average away the idiosyncratic timing of firm-level news arrival. If not, this can lead to large return reversals in the IPT curves, which can produce *IPT* values that cannot be clearly interpreted as timeliness of price discovery. Unfortunately, prior literature does not discuss such problems in great detail. Thus, to ensure that the IPT metric, used in this thesis, is appropriate for making inferences, I examine the impact of such return reversals on both firm-level and portfolio-level *IPT,* in appendix B. I create a proxy to capture the extent of return reversals and find that the firm-level *IPT*, within my setting, is particularly prone to such reversals. However, I find that the portfolio-level metric substantially reduces the impact of return reversals through averaging. In particular, using simulation analysis, I explore portfolio sizes that will mitigate these issues and use this to inform my analysis.

Unlike H1, I am not restricted to management-forecast-issuing firms; hence, the H2 sample differs from and is larger than the H1 sample. I use a coarsened exact matched sample of derivative users and non-users to control for operational uncertainty, the richness of the information

[1] See sections 3.2.3.1 and B.1 for reviews of literature using the portfolio- and the firm-level metrics, respectively.

environment and the sign of the 12-month buy-and-hold return. Given that the impact of the confirmatory role of financial reports on *IPT* may be more important for management-issued good news than bad news, the impact of fair value accounting exposure on *IPT* may be asymmetric for positive and negative intraperiod return observations.

Using a portfolio analysis, I find null results in the positive intraperiod return subsample and results in the opposite direction from that predicted in the negative intraperiod return subsample. These findings, which persist in the constant derivative sample, suggest that fair value accounting does not impede timely price discovery, but rather, can enhance timely price discovery in negative return periods. However, it is also possible that these results are driven by enhanced transparency of derivative use after SFAS 133, which can enhance, rather than deteriorate, the confirmability of financial reports. Alternatively, results may be confounded by Regulation Fair Disclosure (Reg FD). As I discuss, in section 3.2, while I have no reason, ex ante, to suspect any differential impact of Reg FD on derivative users and non-users, I cannot completely dispel its effects. If Reg FD increases the frequency and/or timeliness of voluntary disclosures more for derivative users than for non-users, this would counteract any effects of SFAS 133 on derivative users. Finally, the null results in the positive return subsample may be due to an insufficient sample size for adequately averaging away firm-level returns.

This thesis comprises five chapters, organized as follows. Chapter 2 discusses the confirmatory role of accounting, reviews related literature, and develops the hypotheses. Chapter 3 presents the research design and the sample selection. Chapter 4 provides the results of empirical analyses. Finally, chapter 5 concludes the thesis.

Chapter 2

Literature Review and Hypotheses Development

2.1 Introduction

This chapter discusses relevant literature and develops my hypotheses. The first part of Section 2.2 introduces the confirmatory role of financial reporting and provides the basis for the thesis's theoretical cause and effect. The latter part of section 2.2 discusses relevant literature. Section 2.3 discusses the literature examining fair values. Finally, section 2.4 develops the hypotheses.

2.2 The Confirmatory Role of Financial Reporting

2.2.1 The Confirmatory Role of Financial Reporting - Theory

As I discuss in greater detail in section 2.2.2, prior literature (Gigler and Hemmer 1998; Stocken 2000; Ball 2001; Lundholm 2003) illustrates that, under the confirmatory role of financial reporting, the primary role of financial reports is to provide stakeholders with an ex-post mechanism for validating the accuracy or truthfulness of managers' non-verified, but timely, voluntary disclosures. Importantly, the audit of financial reports adds credence to the financial statement numbers, enabling them to serve a confirmatory role. The expectation of this ex post check motivates managers to truthfully report voluntary forward-looking information. Thus, it promotes an environment where managers can credibly communicate private information that is not ex ante verified.

An implicit assumption underlying the confirmatory role of financial reporting is that a moral hazard problem exists between managers and stakeholders due to information asymmetry. This can lead managers to misreport information communicated to stakeholders. Under the confirmatory role, financial reports alleviate such information problems between managers and stakeholders by motivating and enabling managers to credibly communicate relevant information in the non-directly verified voluntary disclosures. This contrasts with the view that the primary role of mandatory audited financial reports is to provide stakeholders with new valuation information. Under the confirmatory role, financial reports themselves need not communicate new information. Rather, they enable other information channels to credibly communicate relevant information on a timely basis by serving as a source of reliability for financial information in capital markets.

To clarify, the term 'confirmatory role' in this literature should not be linked to 'confirmatory value' in SFAC No. 8, QC7, 9 (FASB 2010). While, in theory, their definitions are similar, conceptualizing 'confirmatory role' as a characteristic of relevance, as in SFAC No.8, may be misleading for purposes of this paper. In particular, I posit that although 'confirmatory value' may be a characteristic of relevance, it critically depends on reliability since unreliable numbers cannot confirm prior information. See Bauer et al. (2014) for a discussion of how "reliability makes accounting relevant." This link to reliability is not conceptualized in SFAC No.8 and, therefore, the framework's reference to 'confirmatory value' is not necessarily consistent with the use of 'confirmatory role' in literature. For example, the change in the fair value of derivatives in the current year has 'confirmatory value' for prior predictions of the fair value change, in the FASB sense. However, the change in the fair value of derivatives may not serve a strong 'confirmatory role' as discussed in the confirmatory literature if the fair value measurement is not based on observable market prices.

2.2.2 The Confirmatory Role of Financial Reporting – Related Literature

The confirmatory role of financial reporting is initially examined in analytical studies such as Sansing (1992), Gigler and Hemmer (1998), Stocken (2000), Lundholm (2003), and Şabac and Tian (2015). In these studies, a key assumption is that verifiable information in financial reports can, to some extent, confirm the truthfulness of unverified information (i.e., voluntary disclosures). Gigler and Hemmer (1998) examine the association between the frequency of mandatory financial disclosures and managers' incentives to issue voluntary disclosures within the context of the confirmatory role of financial reports. They illustrate that increasing the frequency of mandated financial reports promotes the view that financial reports are a primary source of information, to the detriment of the confirmatory role of financial reports. Hence, frequent reporting can negatively affect investors by crowding out more timely and potentially more informative voluntary disclosures. They state (p.118), "…evidence that most of the "news" in earnings has been preempted may actually be evidence of a well-functioning disclosure regime rather than evidence that the regime needs fixing." Under this view, smaller news content in earnings announcements can be a positive outcome. This is in stark contrast to the view that mandatory financial reports serve as a primary source of new information, where larger, not, smaller news in earnings is perceived as a desired outcome.

Sansing (1992), Stocken (2000) and Lundholm (2003) examine the disciplining role of financial reports on managers' voluntary disclosures. Specifically, Sansing (1992) states that the accounting system constrains management from misrepresenting information in management forecasts by serving as a verification mechanism for the truthfulness of the management forecasts. He qualifies that the extent to which the accounting system constrains management forecasts depends on the degree to which the accounting system reflects the private information considered in the forecast. Stocken (2000) and Lundholm (2003) illustrate that the agent truthfully reveals his private information if, among other conditions, the financial report can sufficiently verify the truthfulness of the voluntary disclosure. Financial reports must be able to confirm the earlier voluntary disclosures in order to enable investors to detect and punish dishonest reporting ex post. If investors cannot detect dishonest reporting, the rational, but unethical, manager has no incentive to report relevant forward-looking information truthfully. These models emphasize the critical role that accounting serves in establishing credible communication of private information through voluntary disclosures.

An important aspect of the information environment embedded within the confirmatory theory is the interaction between mandatory financial reports and voluntary disclosures. In the analytical studies discussed above, financial reports serve a critical disciplining role on managers' voluntary disclosures, which, in turn, facilitate the timely communication of relevant information. Accordingly, Ball (2001) argues that an economically efficient financial reporting and disclosure system is one where timely disclosures facilitate informational efficiency and financial reports facilitate contracting efficiency. That is, the communication of relevant new information primarily rests with voluntary disclosures. The role of financial reports, then, is to provide verifiable information that establishes the credibility of voluntary disclosures. Hence, under this view, the financial reports, themselves, cannot effectively satisfy both the relevance and reliability criteria. However, by focusing on reliability, financial reports can promote timely relevant and reliable information in capital markets, when other information channels are considered. Highlighting this interaction, Ball (2001) urges that the impact of accounting be examined in light of other sources of information, rather than in isolation.

In addition to the analytical studies discussed above, archival studies test the confirmatory theory by examining the impact of accounting or audit quality on voluntary disclosure quality. I specifically discuss Beniluz (2005), Ball, Jayaraman and Shivakumar (2012a), Ball, Jayaraman and Shivakumar (2012b) and Frankel, Kalay, Sadka and Zou (2017) and explain how my work contributes to this literature.

My thesis closely relates to Beniluz (2005), which explores the relation between accounting quality and management and analyst forecast characteristics. Consistent with the confirmatory theory, he argues that high quality accounting information - defined as verifiable and auditable information - enables investors to detect bias in management and analyst forecasts. In turn, this increases managers' and analysts' costs of misreporting information and biasing the forecasts. Using five-year absolute discretionary accruals and restatements as inverse proxies for accounting quality, he finds that firms with poorer accounting quality have more optimistic forecasts. In addition, he finds that investors discount their reaction to forecasts of poorer accounting quality firms.

Although the first part of my thesis that examines the impact of accounting on the credibility of voluntary disclosures is similar to Beniluz (2005), it differs in the following way. Beniluz (2005) captures the firm's choice of accounting quality, within a (presumed) static regime of accounting standards. As discussed in Beniluz (2005), poor accounting quality reduces managers' costs of misreporting in their voluntary disclosures because such misrepresentations are harder to detect when accounting quality is low. In other words, poor accounting quality dampens the confirmability of the financial reports. However, poor accounting quality is not costless. Specifically, the firm's choice of low accounting quality, measured as either the magnitude of discretionary accruals or restatements, *increases* litigation costs associated with misreporting mandatory financial reports. So, when firms reduce their accounting quality, they reduce the costs of misreporting in voluntary disclosures at the expense of increased costs of misreporting on financial reports. In contrast, I am interested in capturing the impact of a change in accounting – namely, fair value accounting. The importance of this difference is that fair value accounting alters the managers' *GAAP-permitted* discretion over the accounting numbers. As I discuss in section 2.4.1, such discretion associated with fair values reduces the confirmability of financial reports without necessarily imposing greater litigation costs to using managerial discretion on the reports.

Ball et al. (2012a) examine the confirmatory role of accounting by investigating how a stronger commitment to independent verification enhances disclosure credibility. They use abnormal audit fees based on an audit fee model to proxy for the level of commitment to independent verification, arguing that greater abnormal audit fees reflect a greater resource allocation to financial statement verification. They find that higher audit fees are associated with higher quality (i.e., more frequent, specific, timely and accurate) management forecasts. They also find that firms with higher abnormal audit fees have stronger investor reactions to management forecasts, in the form of higher abnormal returns and abnormal volume, than those with lower abnormal audit fees. They interpret this evidence as documenting a complementary relation between audited financial reports and voluntary disclosures and warn against drawing inferences exclusively from earnings announcement returns.

Frankel et al. (2017) employ an exogenous shock to mandatory reporting quality to study the complementary relation between voluntary disclosures and mandatory reporting quality suggested by the confirmatory role of accounting. Specifically, they use the demise of Arthur Anderson (AA) in 2002 to examine the non-voluntary auditor switch for former AA clients. They find that these firms have lower abnormal accruals post-switch, which they interpret as increased financial reporting quality. Consistent with the confirmatory theory, they find that the voluntary disclosure quality (i.e., frequency, specificity, precision and horizon) of former AA clients improved more than that of non-AA firms in the same period. Further, they find that the magnitude of the investor reaction to management forecasts, controlling for the management forecast surprise, is greater for former AA clients after the auditor switch, indicating an incremental improvement in the credibility of management forecasts for AA client firms relative to other firms.

My thesis differs from Ball et al. (2012a) and Frankel et al. (2017) in that it examines the impact of accounting rather than audit quality. In particular, while greater audit quality can significantly enhance the financial reports' ability to serve a confirmatory role by increasing the accuracy of the reported numbers, the extent of this impact may be limited by the amount of measurement error permitted by the accounting standards. As I discuss in section 2.4.1, fair value accounting is inevitably associated with greater measurement uncertainty, which cannot always be alleviated by an audit, given the nature of fair values.

Also, an incremental contribution of my thesis relative to Beniluz (2005), Ball et al. (2012a) and Frankel et al. (2017) is that my thesis examines the impact of accounting on the timeliness of information within a given period, a construct that encompasses all information sources within that period. The timeliness construct allows one to assess how accounting may affect the speed with which information is incorporated into prices within a given period. The 'speed' provides insight into how well a firm is able to credibly communicate private information on a timely basis. This analysis examines the assertion by Gigler and Hemmer (1998) that a well-functioning system is one where little news remains to be conveyed by the annual earnings announcement as most of the news has been pre-empted via voluntary disclosures.

Finally, Ball et al. (2012b) examine the association between mark-to-market (MTM) accounting for trading securities and information asymmetry in banks. While the primary focus of their study is not the confirmatory role of accounting, they refer to the confirmatory theory as one of their arguments for why MTM accounting may reduce information asymmetry. Specifically, they argue that MTM accounting reduces investors' ability to verify the truthfulness of mangers' earnings forecasts and, thus, makes it more costly for managers credibly communicate private information. They argue that this, in turn, can reduce the likelihood of management forecasts and, thus, exacerbate information asymmetry. They predict and find that banks with trading securities have lower bid-ask-spread, analyst following and IPT and are less likely to issue management forecasts than those without trading securities. Furthermore, using a difference-in-differences research design around SFAS 115 and SFAS 159, they find evidence suggesting that MTM accounting increases bid-ask spreads in banks, relative to historical cost accounting.

While Ball et al. (2012b) examine the impact of accounting on IPT, similar to my thesis, a few differences remain. First, their analysis using IPT is restricted to a cross-sectional test where they compare banks with and without trading securities in the post-SFAS 115 period only. In contrast, I use a difference-in-differences research design, which allows me to draw causal inferences. While they use a difference-in-differences research design around SFAS 115 and SFAS 159, as discussed above, they only examine the bid-ask spread under this analysis and not IPT. Second, their tests use a firm-level IPT metric, which can be potentially problematic for interpreting the timeliness of price discovery, as I discuss in section 3.2.3.1 and appendix B. In this thesis, I use a portfolio-level IPT metric, which alleviates such concerns. Third, they focus on the effects of SFAS 115 in banks, while I examine the impact of a different fair value accounting standard, SFAS 133, in non-financial industries. It is not obvious, ex ante, whether their findings will generalize to SFAS 133 and non-financial industries.

2.3 Fair Value Accounting – Related Literature

Accounting standards have transitioned towards greater fair value accounting in the last four decades, despite on-going controversy about the trade-off between relevance and reliability.[2] Proponents of fair value accounting argue that fair values provide incremental relevant information for the users of financial reports. For instance, Barth (2006, pp.283-284) argues that incorporating more estimates of the future in the financial reports can improve the information available to the users of the financial reports. On the other hand, opponents of fair value accounting raise concerns over fair values' reliability. Ball (2006, p.13) argues that, when fair values are not publicly observable, fair value accounting provides greater opportunity for managers to strategically manipulate their earnings.

Mirroring the fair value debate, the fair value literature can be categorized into two broad streams. The first stream of literature focuses on the gain in relevance associated with fair values. A large portion of this literature (e.g., Barth 1994; Eccher, Ramesh and Thiagarajan 1996; Song, Thomas and Yi 2010) comprises value relevance studies. These studies generally conclude that fair values provide more value-relevant information to investors than historical values, but that measurement uncertainty affects the extent of this informativeness (see Landsman 2007 for a summary). It is worthwhile to note that value relevance studies do not, nor are they intended to, consider the potential interaction between financial reports and voluntary disclosures.

A second stream of fair value literature focuses on the loss in reliability of fair values. This literature argues that greater measurement uncertainty associated with fair values can increase managers' opportunity to bias the reported numbers, adversely affecting their reliability. For

[2] In SFAC No.8, QC6, the FASB defines financial information to be relevant if "it has predictive value, confirmatory value, or both." To be clear, for purposes of this thesis, my use of the term 'relevance' incorporates the quality of predictive value, which is consistent with the spirit of its use in a fair value setting, rather than confirmatory value.

example, Nissim (2003) documents that banks' overstatement of loan fair values is associated with incentives to favorably influence market perceptions of firm risk and performance (e.g., regulatory capital, asset growth, loan portfolio credit quality). Also, using a sample of firms whose book-to-market ratios are likely indicative of goodwill impairment, Ramanna and Watts (2012) find that the decision not to recognize the impairment is associated with managers' contracting and reputational concerns, consistent with opportunism.

As evident in the two streams of literature, I posit that the fair value debate reveals differing opinions on the primary role of financial reports. Proponents of fair value accounting generally interpret as a positive outcome that fair values provide incremental information in the financial statements. These interpretations are predicated on the assumption that the primary role of financial reports is to provide new information. In contrast, opponents of fair value accounting, in raising concerns over the reliability of fair values, reveal that they believe that the primary role of financial reports is not necessarily the provision of *new* information, but rather, the provision of *reliable* information.

In summary, the fair value literature indicates that fair values can provide more relevant information to the users of financial reports, but also that they suffer reliability issues, validating the assertions of both proponents and opponents of fair value accounting. However, much of this fair value literature fails to consider the interaction between financial reports and voluntary disclosures and, thus, the impact of fair value accounting on management disclosures outside the financial reports. This thesis specifically considers this interaction by studying the impact of fair value accounting on the credibility of voluntary disclosures and the timeliness of price discovery, in light of the confirmatory role of financial reports.

2.4 Hypotheses Development

2.4.1. Fair Value Accounting and the Confirmatory Role of Financial Reporting

I argue that the transition towards greater fair value accounting changes the nature of the accounting signal in a way that limits its ability to serve a confirmatory role. Lundholm (2003) discusses that the backward-looking nature of financial reports allows these reports to lend credibility to more timely, yet unverified, voluntary disclosures. Accordingly, the ability of accounting numbers to assess the past is essential to the confirmatory role of financial reporting; this is in stark contrast with fair value accounting's mandate to predict the future. Thus, replacement of historical cost accounting with fair value accounting may have altered the extent to which the financial reports serve a confirmatory role. Specifically, I posit that fair value accounting dampens the financial reports' confirmability because it generally permits greater measurement uncertainty, relative to historical cost accounting.

When market prices are not readily observable, fair value measurement involves estimates that depend, to varying extents, on management's choice of models and parameters.[3] In turn, uncertainty about these choices may reduce the accuracy with which financial reports can confirm the earlier voluntary disclosures. The magnitude of such uncertainty can be substantial. For example, based on interviews with 96 high-level audit engagement team members, Cannon and Bedard (2017) report that uncertainty associated with fair values exceeds materiality (five times materiality) in 72 (21) percent of challenging fair value audits. To make matters worse, an audit cannot alleviate such measurement uncertainty issues when fair values are not readily verifiable.

[3] Measurement uncertainty is not unique to fair values. For example, under historical cost-based accounting, expenses such as warranty and bad debt expense involve uncertainty. However, as Lee (2014) asserts, the level of uncertainty involved in estimating some fair values far exceeds that involved under historical cost-based accounting.

An example auditor statement from Cannon and Bedard (2017) illustrates this point well: "No audit adjustment was proposed and no impairment was recorded, but it could have very easily resulted in an impairment by adjusting the assumptions slightly. The main reason the adjustment was not recorded was the level of uncertainty of management's assumptions (i.e., inputs into the model). Neither the audit firm nor management had firm evidence that could support one assumption was better than another" (p. 98-99). Such estimation uncertainty associated with fair values can reduce the financial reports' ability to accurately reveal misrepresentations in voluntary disclosures, even in the absence of management bias.

Furthermore, greater within-GAAP measurement uncertainty associated with fair values provides managers greater discretion to report opportunistically. This, in turn, can render the fair values less reliable than historical values. As discussed in section 2.3, Ramanna and Watts (2012) and Nissim (2003) provide support that less verifiable fair values are prone to management manipulation. Having said this, I note that managers need not act opportunistically in any given period for fair values to impair confirmation. Rather, the *potential* for managers to act opportunistically, combined with uncertainty about actual actions are sufficient to create credibility concerns about voluntary disclosures.[4] Fair values increase this potential by making it more difficult for investors to assess the truthfulness of managers' earlier voluntary disclosures using the accounting numbers on the financial reports. This, in turn, reduces the costs to managers of including misleading information in their voluntary disclosures. Hence, fair value measurement uncertainty reduces the usefulness of financial reports for evaluating the truthfulness or accuracy

[4] Ball (2001, p.174) argues that "earnings management is a problem precisely when it is not clear whether managers are likely to overstate or understate earnings." He states that if the bias is obvious to investors, they would simply back it out.

of the prior voluntary disclosures by providing managers with greater discretion over reported amounts.

Greater discretion can affect the extent to which financial reports can serve a confirmatory role by allowing greater opportunism. Prior literature suggests that firms opportunistically report their earnings to “meet” the earlier forecasts. For instance, Kasznik (1999) finds that when the firm’s reported earnings fall below the forecasted earnings, managers use positive discretionary accruals to improve the perceived forecast accuracy. However, he does not find symmetric evidence when the reported earnings are above the forecasted earnings, suggesting that managers use discretionary accruals opportunistically to meet earnings targets when the actual earnings falls short. Also, comparing fraud-period reported earnings to actual restated earnings, Baginski, McGuire, Sharp and Twedt (2015) find that firms manage their reported earnings to reduce the perceived management forecast bias. Together, prior findings of opportunistic fair value reporting and earnings management to “meet” forecasted earnings suggest that managers can use the measurement uncertainty associated with fair values to bias the financial reports, reducing the financial reports’ confirmability. Overall, fair value accounting makes it more difficult for investors to assess the truthfulness of managers’ voluntary forward-looking disclosures, relative to historical cost accounting.

2.4.2 Fair Value Accounting and Credibility of Voluntary Disclosures (H1)

Beniluz (2005) states that the credibility of voluntary disclosures rests heavily on the extent to which the accounting signal provides an effective ex post settling-up mechanism and, thereby, is able to serve a confirmatory role. Section 2.4.1 argues that, relative to historical values, fair values may not be as well-suited to serving a confirmatory role because they are subject to greater measurement uncertainty. Building on this argument, I posit that fair value accounting adversely affects voluntary disclosure credibility. Hence, I hypothesize that:

Hypothesis 1 (H1). *All else equal, greater exposure to fair value accounting reduces the credibility of voluntary forward-looking disclosures.*

H1 is not without tension. As discussed in section 2.4.1, managers may use accounting discretion to "meet" expectations communicated in prior voluntary disclosures (Kasznik 1999; Baginski et al. 2015). If investors fail to identify such behavior, they may perceive these disclosures as more credible, not less. Baginski et al. (2015) find some support for such behavior examining fraud periods where earnings were managed to produce an appearance that the earlier forecasts were less optimistic and more accurate than they actually are. Specifically, their results suggest that, relative to the pre-fraud period, investors perceive fraud period bad news forecasts as more, not less, credible.

In addition, to the extent that fair values on the financial reports are reliable and accurate, fair value accounting can potentially allow investors to confirm voluntary forward-looking disclosures earlier than under historical cost accounting. Note that under historical cost accounting, investors have to wait until the outcome is realized (e.g., sale of asset), whereas fair value accounting provides interim values of assets and liabilities. Fair value's ability to confirm forward-looking disclosures earlier than historical cost accounting may strengthen the market reaction to voluntary disclosures.

2.4.3 Fair Value Accounting and Timeliness of Price Discovery (H2)

If we consider that all information will eventually be released to the market, it is *timeliness* that affects the information's decision-usefulness. Healy and Palepu (2001, p.407) state that the role of disclosure is to mitigate the "information and incentive problems [that] impede the efficient allocation of resources in a capital market economy." More timely information equips investors with a more comprehensive information set at any given point in time and, therefore, should lead to a more efficient allocation of capital and a more accurate valuation of equity. Hence, timeliness is an important aspect of informational efficiency. By examining an information timeliness construct that encompasses the consideration of information both *within* and *outside* of financial reports, this thesis provides insight into how fair value accounting affects investors via the interaction between financial reports and voluntary disclosures.

Fair value accounting may affect the timeliness of price discovery via its impact on the confirmability of financial reports and, thus, on the credibility of voluntary disclosures. Pownall and Waymire (1989) argue that investors will discount news that they perceive to be less than fully credible and that the extent of the discount will be decreasing in the perceived credibility. Indeed, Rogers and Stocken (2005) find that investors discount their reaction to management forecasts in accordance with the predictable bias in the forecast, which is a function management's incentives to misreport and the market's ability to detect misrepresentation - both determinants of credibility. Further, Ng et al. (2013) find that less credible management forecasts have smaller market reactions around their issuance and a larger post-issuance drift. Hence, less credible voluntary disclosures will likely suffer dampened market reactions.

In addition, given disclosure costs, managers may be less likely to issue voluntary disclosures when they perceive that investors will discount these disclosures. In support of this conjecture, Lennox and Park (2006) find that managers are less likely to issue management forecasts when the expected reaction to each unit of news in the forecast is lower. Lennox and Park (2006) operationalize the expected reaction to each unit of news in the forecast using the prior *earnings* response coefficient. Thus, this conclusion is premised on the assumption that the earnings response coefficient is an appropriate proxy for the expected management forecast response coefficient.

Collectively, these studies suggest that less credible voluntary disclosures reduce the timeliness of price discovery because investors will reduce the magnitude of their reactions to less credible disclosures and, this, in turn, makes managers less inclined to issue voluntary disclosures.[5] Therefore, building on the discussion in Sections 2.4.1 and 2.4.2 that fair value accounting will dampen the financial reports' confirmability and, thereby, reduce voluntary disclosure credibility, I hypothesize that:

Hypothesis 2 (H2). *All else equal, greater exposure to fair value accounting reduces the timeliness of price discovery.*

While the confirmatory theory predicts H2, the tension in this hypothesis derives from the fact that fair values generally include more current information than historical cost. Therefore, to the extent that they are credible, fair values in interim financial reports may increase the timeliness of price discovery. This biases against finding results supporting H2.

In both hypotheses, I examine the effects of greater exposure to fair value accounting on constructs that involve investor pricing (i.e., credibility of voluntary disclosures, timeliness of price discovery). While different systems of accounting (e.g., historical cost and fair value) provide different types of information on financial reports, stock prices ultimately reflect investors' aggregate beliefs about the expected future payoffs to shareholders. So, under either accounting

[5] I examine the impact of an increase in exposure to fair value accounting on the frequency of management forecasts in additional analyses, in section 4.3.3.2.

system, what is ‘priced’ by investors is information about expected future economic profits, revealed through accounting numbers.

Chapter 3

Research Design and Sample

3.1 Introduction

This chapter presents the research design for each of my hypotheses, with its associated sample design. Section 3.2 presents the research design, beginning with an overview of the difference-in-differences research design. I then describe the detailed research design for each hypothesis separately. Section 3.2.1 discusses the identification of derivative users and non-users. Section 3.2.2 presents the research design to test the impact of exposure to fair value accounting on the credibility of voluntary disclosures (H1), while section 3.2.3 presents the design for the impact on the timeliness of price discovery (H2). Section 3.3 presents the sample selection process for H1 and H2, in subsections 3.3.1 and 3.3.2, respectively.

3.2 Difference-in-Differences Research Design

To examine the impact of exposure to fair value accounting, I exploit SFAS 133 - *Accounting for Derivative Instruments and Hedging Activities* (FASB 1998) (currently ASC 815), which increases fair value accounting exposure for derivative users by mandating all derivatives to be reported at fair value. In tests of both H1 and H2, I use a difference-in-differences (DiD) research design, comparing derivative users to derivative non-users, pre- versus post-mandatory adoption of SFAS 133.

SFAS 133 has two advantages for capturing the impact of an increase in exposure to fair value accounting. First, it mandates fair values that often include some degree of measurement uncertainty, which I hypothesize can dampen the confirmability of financial reports. For selected financial derivative instruments outstanding at the end of 1999, the Bank for International

Settlements (BIS) (2000) reported total notional amounts for over-the-counter (OTC) derivative instruments and exchange traded instruments of \$88.2 trillion and \$13.5 trillion, respectively.[6] Hence, the vast majority of derivative instruments are not traded on an exchange and their fair values therefore involve some level of measurement uncertainty.

Second, SFAS 133 results in frequent bi-directional fair value changes. Relative to uni-directional fair value changes, which only affect accounting numbers in one direction, bi-directional changes provide greater opportunity for managers to manage accounting numbers on the financial report. For example, goodwill impairment is a uni-directional fair value change that can decrease, but not increase earnings. While management has some discretion over the timing and/or the amount of the impairment, this fair value adjustment cannot be used to increase earnings. In contrast, bi-directional fair value changes, such as fair value gains/losses on derivative instruments, can both decrease or increase earnings. Thus, I argue that both measurement uncertainty and frequent bi-directional fair value changes can provide management with greater discretion to opportunistically bias financial reports, weakening the reports' confirmability.

Prior to SFAS 133, the accounting for derivative financial instruments was primarily governed under SFAS 52– *Foreign Currency Translation* (FASB 1981), SFAS 80– *Accounting for Futures Contracts* (FASB 1984a) and the Emerging Issues Task Force (EITF) Issue No. 84-36 – *Interest Rate Swap Transactions* (FASB 1984b). However, the accounting guidance for derivative financial instruments was incomplete as these standards only address a few instruments – namely exchange-traded futures contracts, foreign currency forward contracts, and interest rate

[6] Selected exchange-traded instruments include interest rate futures, interest rate options, currency futures, currency options, stock market index future and stock market index options. Selected OTC instruments include interest rate swaps, interest rate options, currency swaps, currency options, foreign exchange forwards and swaps and equity and commodity instruments (see BIS 2000, p.30, table II.3.2).

swaps. For derivative instruments covered in these pronouncements, those used for speculative purposes were required to be reported at fair value, while those used for hedging purposes were required to be reported in the same manner as the hedged asset or liability. For example, derivative instruments used to hedge fair-valued assets or liabilities, such as foreign currency available-for-sale securities, were required to be reported at fair value. In contrast, those used to hedge assets or liabilities at historical or amortized costs (hereafter, historical cost), such as held-to-maturity debt, were reported at historical cost, which was often negligible or zero.[7] SFAS 133 (FASB 1998, par. 235) states that "The EITF addressed the accounting for some derivatives and for some hedging activities not covered in either Statement 52 or Statement 80. However, that effort was on an ad hoc basis and gaps remained in the authoritative literature." It goes on to explain that "[t]he result was that (a) many derivative instruments were carried "off-balance-sheet" regardless of whether they were formally part of a hedging strategy, (b) practices were inconsistent among entities, and (c) users of financial reports had inadequate information." Thus, reporting of derivatives at fair value was limited in the pre-SFAS 133 period.

SFAS 133 was issued to provide more complete and uniform accounting guidance for derivative instruments. It mandates recognition of all derivatives at fair value and standardizes the hedge accounting criteria. The accounting treatment for a derivative depends on its intended use. If a derivative is designated as a hedge of exposure to changes in fair value (i.e., fair value hedge), the unrealized fair value gain or loss is recorded in net income. If a derivative meets necessary conditions and is designated as a hedge of exposure to variable cash flows (i.e., cash flow hedges) or of foreign currency exposure of a net investment in a foreign operation, the unrealized fair value

[7] In the pre-SFAS 133 period, fair-valued assets and liabilities generally comprised trading and available-for-sale securities.

gain or loss is recorded in other comprehensive income (OCI). Any ineffective portion of the gain or loss on a derivative designated as a hedging instrument is included in net income. Finally, if a derivative is not designated as a hedging instrument, the unrealized fair value gain or loss is recognized in net income. By mandating fair value reporting for all derivatives, SFAS 133 increased the exposure to fair value accounting for derivative users.

For H2, the pre-period is the fiscal year two years prior to the first fiscal year for which SFAS 133 is mandatorily effective (hereafter, mandatory fiscal year), and the post-period is the mandatory fiscal year. I exclude the one year prior to the mandatory fiscal year, to reduce the likelihood of capturing potential anticipatory effects or early adopters. Since SFAS 133 is mandatorily effective for fiscal periods *beginning* after June 15, 2000, fiscal years *ending* June 2001 to May 2002 (fiscal 2001) comprise the first mandatory fiscal year.[8] Accordingly, for H2, the pre-period includes fiscal 1999 and the post-period includes fiscal 2001.

For H1, which suffers greater data limitations as discussed below, I use two years for each of the pre- and post-periods to maximize the sample size. In addition, using more than one observation for a given firm in each of the pre- and post-periods guards against firm-level idiosyncratic effects related to the management forecast announcement returns. However, errors may be correlated between observations for a given firm, which can lead to unreliable regression results. I address such concerns by clustering the standard errors at the firm level, as I discuss in section 3.2.2, and using firm fixed effects in the additional analyses, reported in section 4.2.3.1.

[8] This standard was originally effective for fiscal years beginning after June 15, 1999. The date was later amended under SFAS 137 (FASB 1999).

For H1, the pre-period includes fiscal 1998 and 1999 and the post-period includes fiscal 2001 and 2002.

I identify a firm as a derivative user if it holds any derivatives in the latest pre-period year, fiscal 1998 or 1999 for H1 and fiscal 1999 for H2. Because firms that do not use derivatives are not affected by SFAS 133, I use them as a control group. I discuss the identification of derivative users and non-users in section 3.2.1. By comparing differences in the credibility of voluntary disclosure or the timeliness of price discovery between derivative users (treatment) and non-users (control), pre- and post- adoption of SFAS 133, I control for time-invariant firm characteristics. The DiD research design also controls for confounding concurrent market-wide factors, which can influence market reaction to disclosures or the timeliness of price discovery, providing a strong test of causal effects. For instance, changes in technology (e.g., easier access to internet) can significantly alter the transmission of information, which, in turn, affects the timeliness of price discovery for all firms. Comparison of treatment to control firms measured over the same time period should mitigate the effects of market-wide factors. In sum, a DiD research design provides a strong test of causal effects. However, several key identifying assumptions are required to draw causal inferences from a DiD analysis. I discuss these below.

First, treatment and control firms should not differ fundamentally. Ideally, I would like to randomly assign treatment to a homogeneous group of firms. However, this is not feasible because the applicability of SFAS 133 depends on whether or not firms use derivatives.[9] Firms use derivatives to either take risk (i.e., speculate) or manage risk (i.e., hedge). Thus, derivative users

[9] Alternatively, I can compare treatment firms with greater derivative use to those with less. Firms with greater derivative use will likely experience a larger increase in exposure to fair value accounting. Unfortunately, it is difficult, if not impossible, to assess the extent of derivative use from examining the notes to the financial statements in the pre-SFAS 133 period, where disclosures around derivative use was often minimal.

likely differ from derivative non-users in their risk exposures and/or risk management practices, which, in turn, affect operational uncertainty.

To alleviate concerns that control and treatment firms differ fundamentally, I use matched samples of control and treatment firms that control for the major confound, operational uncertainty, as I discuss in sections 3.2.2 and 3.2.3. The matched sample is less subject to the concern that treatment and control firms differ fundamentally. However, the trade-off is a smaller sample size, which may reduce test power. In addition, for H1, I test for differences in the control variables between treatment and control groups. Where statistically different, I include the control variables in the regressions. Furthermore, for H1, I include firm fixed effects, in additional analyses, to control for time-invariant differences between treatment and control firms. For H2, I use alternative matched samples to control for potential confounds that statistically differ between the two groups.[10] If there remain fundamental differences between control and treatment firms affecting the credibility of management forecasts and/or the timeliness or price discovery that I fail to capture, the generalizability of the results may be limited to derivative users (i.e., treatment firms).

A *second* key identifying assumption in a DiD design is that the allocation of treatment must be exogenous. This assumption is not met in my setting because firms can potentially self-select into or out of treatment by acquiring or disposing of derivative instruments. However, I highlight that the primary motivation for derivative use in hedging firms should be to manage risk exposures.[11] In section 4.2.2.1 and 4.3.2.1, I find that 4.8% and 5.5% of treatment firms stop and

[10] Recall that the test of H2 uses a portfolio analysis, rather than a regression analysis.

[11] There are very few speculating firms in my sample, at least based on the pre-period note disclosures. Out of the 175 derivative users in my random sample of 250 observations for manual tracing to the 10-k filing in appendix C (table C.2), only 4 firms indicate that they use derivatives for speculative purposes.

45.4% and 17.1% of control firms begin using derivatives in the post-period in the H1 and H2 samples (hereafter, switching firms), respectively. These firms may be influenced to switch their decision to use or not use derivatives because of the provisions under SFAS 133, as I discuss in section 4.2.2.1. In these cases, the allocation of treatment is not exogenous.

To alleviate concerns that these switching firms confound results, I also test H1 and H2 using constant subsamples of treatment/control firms that continue to use/not use derivatives in the post-period (hereafter, constant derivative samples). This is more consistent with prior literature examining derivative users and non-users (e.g., Donohoe 2015; Chang et al. 2016; Campbell, Cao, Chang and Chiorean 2020), which restricts the non-user control firms to those that do not use derivatives throughout their sample period. For the firms that continue to use or not use derivatives throughout the sample period, I argue that SFAS 133 is exogenous because their decision to use or not use derivatives is not influenced by the application of the standard itself. These decisions were made in the pre-period, apart from the influence of the standard. In particular, I exclude the year immediately prior to mandatory application of the standard (i.e., fiscal 2000) to minimize any anticipatory effects or early adopters.

Third, the DiD research design requires that the composition of treatment and control groups be stable throughout the sample period. As discussed above, I find that firms switch into or out of treatment by acquiring or disposing derivative instruments, leading to unstable control and treatment groups. When treatment firms cease to hold derivatives in the post-period, they are similar to control firms in that they do not experience an increase in exposure to fair value accounting. In addition, they experience a change in their operations due to derivative non-use. Conversely, control firms that begin using derivatives in the post-period are also problematic because these control firms are exposed to the treatment (i.e., increased exposure to fair value accounting). I discuss the potential reasons for the change in derivative use as well as the impact of including changing firms in greater detail in sections 4.2.2.1 and 4.3.2.1.

Using the constant derivative samples helps to alleviate concerns over non-constant treatment and control groups. However, the trade-off in using the constant derivative samples is that the identification of treatment and control firms is based not only on pre-period attributes, but also on post-period attributes, which may be influenced by SFAS 133. Hence, I cannot generalize findings using the constant derivative samples to firms that change their use of derivatives around SFAS 133.

Finally, to appropriately draw causal inferences, any contemporaneous events affecting the post-period must affect treatment and control groups similarly. Reg FD became effective in October 2000, overlapping with the post-period in this thesis. Reg FD requires that when public companies disclose material information to a limited group of individuals (e.g., financial analysts), they must also publicly disclose this information. Essentially, this regulation was implemented to prohibit selective disclosure, while motivating more timely public disclosure. Consistent with this aim, Heflin, Subramanyam and Zhang (2003) find evidence suggesting that Reg FD substantially increased the frequency of management forecasts.

I have no reason, ex ante, to suspect that Reg FD will affect treatment and control firms differently. In particular, any size or industry differences, which may moderate the impact of Reg FD on disclosure credibility or intraperiod timeliness, are eliminated in the matched samples. Furthermore, in section 4.3.3.3, I find no DiD in analyst following between control and treatment firms from the pre- to the post-periods. Nevertheless, I cannot effectively rule out potential confounding effects of Reg FD.

While the DiD research design described in this section applies to tests of both H1 and H2, a key difference between H1 and H2 necessitates a separate discussion of the detailed research design for each hypothesis. Because of specific characteristics of the intraperiod timeliness metric used as a proxy for timeliness of price discovery, which I discuss in section 3.2.3.1, H2 must be tested at the portfolio level. H1, not being subject to this constraint, is tested at the firm-year level. This difference leads to distinct research designs for H1 and H2, which I discuss in sections 3.2.2 and 3.2.3, respectively. Prior to describing these research designs, I discuss the identification of derivative users and non-users in section 3.2.1.

3.2.1 Identification of Derivative Users and Non-Users

As discussed in section 3.2, I identify a firm as a derivative user if it holds derivatives in the latest pre-period year. To assess whether a firm holds derivatives, I use a combination of keyword search results via SeekEdgar and manual data collection by examining the 10-K filing on SEC's EDGAR database.[12] Prior literature (e.g., Zhang 2009; Donohoe 2015; Chang, Donohoe and Sougiannis 2016) uses a keyword search to narrow down the set of potential derivative users, which they then manually verify by examining the 10-K filing.[13] I follow this prior research in using a keyword search to facilitate the identification of derivative users.

Specifically, I use a *count* of keywords to distinguish derivative users from non-users, following Campbell et al. (2020). In the pre-SFAS 133 period, SFAS 119 – *Disclosure about Derivative Financial Instruments and Fair Value of Financial Instruments* (FASB 1994)

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[12] I use the SeekiNF search engine provided by SeekEdgar to perform the keyword search. See seekedgar.com for more details. See the following link for SEC's EDGAR database: https://www.sec.gov/edgar/searchedgar/companysearch.html

[13] Compustat provides data on derivative gains/losses that flow through OCI (AOCIDERGL) beginning in 2001 and data on ineffective gains/losses on hedges (HEDGEGL) beginning in 2005. However, these data do not identify all derivative users.

mandates firms to disclose the contract or notional amounts and the nature and terms for all derivatives, by category (e.g., class, business activity, risk). Hence, the more derivative words the financial statement notes contain, the higher the likelihood that the firm is a derivative user.[14] I note that the mere presence of a derivative word does not necessarily indicate derivative use. For example, some derivative non-users disclose SFAS 133 as a newly issued, but not yet effective standard and include multiple derivative words in that disclosure. As another example, some derivative non-users use derivative words in combination with negation words to indicate that they do *not* use or hold any derivative financial instruments.

I use a list of 101 derivative-related keywords/phrases (see figure C.1). It is most similar to the list used by Campbell et al. (2020), which is the most comprehensive list used in prior studies, to my knowledge.[15] As I mentioned above, these prior studies manually verify all initially identified potential derivative users. In contrast, to minimize manual data collection costs, I intend to rely solely on results of the keyword search without manual verification, where these results reasonably identify users. Therefore, using a comprehensive list of keywords is necessary to ensure I capture most cases of derivative words.

In addition to the count of keywords, I also incorporate information on the absence or presence of keywords related to specific derivative instruments (hereafter, specific instrument words). Specific instrument words comprise a subset of the derivative words list and include

[14] I observe this to be the case in my random sampling results in appendix C. For example, in a random sample of 50 observations from the H1 and H2 samples with 1-5 derivative words, I find that 28% are derivative users. In the random sample of 50 observations with 6-10 keywords, 40% are users. In the random sample of 50 observations with 20 or more derivative words, 100% are users. See table C.2 in appendix C.

[15] Campbell et al. (2020) use a list of more than 100 derivative-related keywords/phrases. The authors kindly provided me with the list of words used in their paper, for reference. Prior studies such as Manconi, Massa and Zhang (2017) use 36 keywords to identify potential interest rate or foreign exchange hedgers. As another example, Zhang (2009) uses 9 keywords to identify potential derivative users.

words such as interest rate swap, foreign exchange forward, and commodity contracts. Firms are less likely to include specific instrument words if they don't hold any derivative instruments, but merely use derivative words to discuss the FAS 133 pronouncement or indicate their non-use of derivatives, as explained above.[16] Rather, these firms are more likely to include only generic derivative words, such as: derivatives, hedge and derivative instruments. Hence, the presence/absence of specific instrument words helps to distinguish such cases.

I begin with Campbell et al.'s (2020) criteria of classifying firms with 20 or more derivative words as derivative users. I then calibrate the criteria to identify derivative users, incorporating the presence/absence of specific instrument words, by manually tracing random subsamples of my H1 and H2 samples to the 10-K filings, by categories of keyword count (e.g., 1-5 keywords, 6-10 keywords, etc.). Based on the results of this analysis, reported in appendix C, I classify a firm as a derivative user if the 10-K has 20 or more derivative words, or if it has 16-19 derivative words and at least one specific instrument word. I classify a firm as a derivative non-user if it has zero derivative words.

For the remaining firms - those with 1-16 derivative words, or between 16 and 19 derivative words and zero specific instrument words – the word counts ambiguously identify derivative users and non-users. Hence, for these firms, I cannot identify derivative use based on the word counts alone. Instead, I manually trace these observations to the 10-K filings on SEC's EDGAR database to assess derivative use. Specifically, I search for the keywords - derivative, hedge, hedging, and swap - and read the surrounding text to assess whether the firm uses

[16] I find this to be the case in section C.3 of appendix C.

derivatives.[17] This usually leads me to the following sections of the 10-K filing: disclosures about risk, summary of significant accounting policies, and any notes pertaining specifically to "derivative financial instruments" or "financial instruments". Where no such words are found, I also browse the summary of significant accounting policies, and any notes pertaining to financial instruments before classifying these observations as non-users. I also manually examine observations with no SeekEdgar results due to missing or miscoded footnote (i.e., notes to the financial statements) headings and suspicious observations where SeekEdgar reports the total words on the report as less than 1000 words.[18]

3.2.2 Research Design - Fair Value Accounting and Credibility of Voluntary Disclosures (H1)

To examine the impact of fair value accounting on voluntary disclosure credibility, I focus on management forecasts as a key voluntary disclosure as they constitute a significant portion of the information communicated outside of financial reports. Ball and Shivakumar (2008) show that, among firms whose managers issue earnings forecasts, those forecasts are associated with approximately one-quarter of quarterly return volatility. I then infer the credibility of management forecasts using the management forecast response coefficient (MFRC). Jennings (1987) states that the market reaction to a management forecast is a function of its news content and the credibility of the news. Hence, the MFRC, which captures the market reaction per unit of news in the forecast, reflects the credibility of the news in the disclosure. Consistent with this idea, Rogers and Stocken (2005) find that the market adjusts its reaction to forecasts in accordance with the predicted bias based on management's incentives to misreport.

[17] I specifically search for the word "swap" even though this is not a generic derivative keyword like derivative or hedge because interest rate swaps are not always described as derivatives.

[18] See Appendix C for a further description of these observations.

I focus on forecasts of annual earnings to capture the confirmatory effect of *audited* financial reports as the independent audit provides the added reliability necessary for financial reports to serve a confirmatory role, as discussed in section 2.2.1. I use earnings per share (EPS) forecasts because the EPS figure is important to investors. Asquith, Mikhail and Au (2005) find that nearly all (99.1%) of the 1,126 sell-side analyst reports issued between 1997 and 1999 examined in their study provide an EPS forecast. Similarly, Previts, Bricker, Robinson and Young (1994), examining 479 sell-side analyst reports issued between 1987 and 1992, find that analysts focus much of their discussion around earnings. Hirst, Koonce and Venkataraman (2008, p.315) state that "Such [earnings] forecasts represent one of the key voluntary disclosure mechanisms by which managers establish or alter market earnings expectations…"

To test H1, I use the following model, which is similar to that used by Rogers and Stocken (2005):

$$\begin{aligned} MF\_CAR_{0,1} = {} & \beta_0 + \beta_1 MF\_SURP + \beta_2 TREAT + \beta_3 POST + \beta_4 TREAT \times POST \\ & + \beta_5 TREAT \times MF\_SURP + \beta_6 POST \times MF\_SURP \\ & + \beta_7\ TREAT \times POST \times MF\_SURP + \Sigma \Gamma X + \varepsilon \end{aligned} \tag{3.1}$$

where $MF\_CAR_{0,1}$ equals the two trading-day cumulative market-adjusted returns around the management forecast date and measures investor reaction to the forecast.[19] The precise definitions of all variables appear in Appendix A and control variables (*X*) are described below.

As discussed above, I infer the credibility of management forecasts from investors' response to the news in the forecast, or MFRC, represented by the coefficient on *MF_SURP* and its interactions. *MF_SURP* represents the magnitude of the news in the forecast, calculated as the management forecast EPS minus the mean analyst forecast EPS in the set of analyst forecasts

[19] Using the three-day cumulative market-adjusted returns does not qualitatively alter results. See section 4.2.3.6.

issued 90 to 2 calendar days prior to the management forecast date, deflated by the pre-management forecast share price. I follow prior literature (e.g., Pownall and Waymire 1989; Williams 1996; Ajinkya, Bhojraj and Sengupta 2005; Anilowski, Feng and Skinner 2007; Choi, Myers, Zang and Ziebart 2010) in using prior analyst forecasts as a proxy for investors' prior earnings expectation when calculating the management forecast surprise. Prior research indicates that analyst forecasts are generally more accurate proxies for prior earnings expectations earnings than time-series models.[20] I only include forecasts issued within 90 days prior to the management forecast date to keep relatively recent analyst forecasts. Using relatively recent analyst forecasts as the benchmark for pre-existing news reduces the chance that *MF_SURP* captures news that is already communicated and priced by the market via other information channels.[21] The less stale news is captured in *MF_SURP*, the better I can infer the credibility of management forecasts from the MFRC.

The main test variable in (3.1) is an interaction of the forecast surprise, *MF_SURP*, with an indicator for treatment firms, *TREAT,* and an indicator for post-SFAS 133 observations, *POST*. As discussed in section 3.2, treatment firms are those identified as derivative users, while control firms are those identified as derivative non-users, in the latest pre-period. If fair value accounting weakens the confirmability of financial reports, I expect to observe a negative coefficient β_7, indicating a more negative change in the MFRC for treatment firms from the pre- to the post-period, relative to the change in control firms over the same period. The coefficient β_5 represents differences in the MFRC between treatment and control firms in the pre-period. Given

[20] Brown and Rozeff (1978), Fried and Givoly (1982) and O'Brien (1988) find evidence suggesting that analyst forecasts are generally more accurate estimates of earnings expectation. They argue that the reason for this finding is that analysts incorporate information beyond historical time series data.

[21] Using varying horizons throughout the year, O'Brien (1988) demonstrates that a more recent analyst forecast is more accurate than a less recent one.

that my research design does not capture an all-else-equal control group because the applicability of SFAS 133 depends on whether or not the firm holds derivatives, I do not make predictions about the sign of β_5. I expect a positive coefficient β_6, which measures differences in the MFRC between the pre- and post-periods for control firms. Heflin et al. (2003) document improvements in the information available to investors, evidenced by stock prices which anticipate a larger proportion of the earnings announcement information and an increase in management forecasts, after Reg FD, which came into effect in 2000. I posit that this, in turn, can enhance the credibility of management forecasts.

A key potential confound for this test is operational uncertainty. Derivatives are often associated with significant volatility and/or uncertainty with regard to their economic benefits, which can make accurate earnings prediction more difficult. In turn, as Rogers and Stocken (2005) argue, forecasting difficulty can dampen investors' ability to assess the truthfulness of managers' forecasts and increase managers' propensity to strategically bias their forecasts. Using non-financial and non-utility industry firms during the period 1998-2011, Chang et al. (2016) find evidence suggesting greater earnings prediction difficulty for derivative users. They find that initiation of derivative use is associated with less accurate and more dispersed analysts' earnings forecasts.

In contrast, if derivatives are effective hedges of risk associated with underlying assets and/or liabilities, derivatives can lead to less operational uncertainty and more, not less, accurate earnings forecasts, which can improve the confirmability of financial reports. Ranasinghe, Sivaramakrishnan, and Yi (2021) find some support for this contrasting argument, examining firms in the oil and gas exploration and production and the airline industries - two industries that use derivatives extensively - in the period 2001-2007. In contrast to the findings of Chang et al. (2016)

discussed above, they find that firms that use derivatives for hedging purposes have *more* accurate and *less* dispersed analyst forecasts, indicating better earnings predictability. However, they find that this positive effect disappears for firms with ineffective hedges and actually turns negative for firms with *only* ineffective hedges.

I control for operational uncertainty by including the volatility of operating cash flows, *OCFVOL*, following Dechow and Dichev (2002), as a control variable. As an alternative way to control for operational uncertainty, I form a matched control group of derivative non-users using one-to-one coarsened exact matching (CEM).[22] I match on *OCFVOL* and Fama-French (1997) 12 industry classifications (FF12) to control for operational uncertainty. In addition, I match on the fiscal year, because the latest pre-period can be 1998 or 1999, and on firm size (*MVE*). I first sort (coarsen) the continuous variables, *OCFVOL* and *MVE*, into bins using the lowest number of equally spaced cutpoints (n) to achieve covariate balance. This ensures covariate balance, while preserving the largest possible sample size. The cutpoints include the extreme outermost values; thus, n cutpoints produce n-1 bins. The CEM algorithm matches each derivative user to a non-user in the same *OCFVOL*-*MVE*-industry-year stratum, without replacement. If the stratum has multiple non-users, the algorithm chooses one at random. Using this matched sample of treatment and control firms provides a stronger control for operational uncertainty than simply including *OCFVOL* as a control variable in a regression using an unmatched sample because it does not require a linearity assumption. However, it reduces the sample size substantially, as I show in section 3.3.1. Accordingly, I examine H1 using both the unmatched sample (controlling for *OCFVOL* by inclusion in the regression) and the matched sample.

[22] I use the Stata command cem developed by Blackwell, Iacus, King and Porro (2009).

While prior literature has often used propensity score matching (PSM) to match derivative users and non-users (e.g., Donohoe 2015; Chang et al. 2016), I choose to use CEM because King and Nielson (2019) show that relative to other matching models, PSM is not efficient at achieving covariate balance. They argue that PSM sometimes increases, rather than decreases, imbalance in some covariates because it prunes observations based on the propensity score, rather than on the covariates themselves. They propose CEM as a preferred alternative to PSM for matching covariates when matching on continuous, discrete and mixed variables. Since I match on both discrete (industry classification, fiscal year) and continuous (*OCFVOL, MVE*) variables, I choose to match treatment to control firms using CEM.

I also consider controls for various management forecast and firm characteristics that have been documented, in prior literature, to influence the market's reaction to the forecast. I consider *MF_SURP* × |*MF_SURP*|, following Lipe, Bryant and Widener (1998) and Rogers and Stocken (2005), to control for a potential non-linear relation between stock returns and earnings news suggested by prior studies. For example, Freeman and Tse (1992) find that the market response to each unit of earnings surprise decreases as the magnitude of the surprise increases. I consider an indicator variable for loss EPS forecasts (*MF_LOSS*) because prior studies like Hayn (1995) and Basu (1997) find that the informativeness of and, thus, the market reaction to losses is lower than that for profits. I include both point and range forecasts, and control for the width of the forecast interval (*MF_WIDTH*) because Baginski, Conrad and Hassell (1993) find that more precise forecasts induce stronger market reactions, consistent with Kim and Verrecchia's (1991) model. I also consider management forecast horizon. Management forecasts that forecast longer horizons are generally less credible due to greater uncertainty pertaining to the prediction of earnings; thus, I consider including *MF_HORIZON* following prior literature (e.g., Bamber and Cheon 1998;

Rogers and Stocken 2005).

In addition, I control for various firm-level characteristics. I proxy for size using the natural log of market value of equity (*MVE*). Bamber and Cheon (1998) argue that size is correlated with the costs of preparing disclosures. Furthermore, Freeman (1987) argues and finds evidence suggesting that larger firms have a richer information environment that anticipates a larger proportion of the earnings information earlier than smaller firms. Hence, larger firms experience smaller announcement returns. I consider controlling for firm growth using *MTB*, as the market reaction may vary with the extent of firm growth. Specifically, Gong, Li and Xie (2009) argue that "the valuation of high-growth firms largely hinges on expected future cash flows…, which intensifies the market demand and public scrutiny for forward-looking information disclosures." I posit that this, in turn, can influence the credibility of the voluntary disclosure.

In addition, I consider controls for management forecasts that are issued concurrently with an earnings announcement (*EA_CONCUR*), to isolate the impact of the management forecast surprise from the concurrent earnings announcement surprise. I also control for the earnings announcement surprise (*EA_SURP*), measured using the rolling seasonal random-walk model following prior literature (e.g., Livnat and Mendenhall 2006; Francis, Lafond, Olsson and Schipper 2007). That is, *EA_SURP* is the current quarter EPS minus the four quarters ago EPS, deflated by the pre-earnings-announcement share price. When the management forecast is not issued concurrently with an earnings announcement, *EA_SURP* equals zero. I also consider controlling for potential non-linearity in the response coefficient to the earnings news ($EA\_SURP \times |EA\_SURP|$) and loss earnings (*EA_LOSS*) for the same reasons explained above for management forecasts.

I transform the continuous control variables other than $MF\_SURP \times |MF\_SURP|$ (e.g., *MF_WIDTH*, *MVE*) into binary variables equal to one for above median values and zero otherwise (e.g., *HiMF_WIDTH* and *HiMVE*). Then, I interact the binary variables with *MF_SURP* to control for their effect on the MFRC. I use binary variables because the interaction between two continuous variables is difficult to interpret.[23] I perform the regression analyses using firm-level clustering and heteroscedasticity-robust standard errors to avoid bias in standard errors resulting from correlated residuals between observations of the same firm (Petersen 2009).

While I consider all of the above variables, I do not include them all in the regressions as controls. Instead, I only include those variables that are statistically different between treatment and control groups in the respective H1 samples. I report the results of the t-tests of the management forecast-level and firm-level variables considered, in table 4.2 in section 4.2.1.2. Based on these results, I exclude variables that do not differ between treatment and control groups to reduce the number of unnecessary regressors in the regression model.

In addition to the regression model in equation (3.1), I allow the MFRC to vary depending on the sign of *MF_SURP* (i.e., good versus bad news) as follows:

$$\begin{aligned}MF\_CAR_{0,1} = \beta_0 &+ \beta_1 MF\_SURP\_GNEWS + \beta_2 MF\_SURP\_BNEWS + \beta_3 TREAT\\ &+ \beta_4 POST + \beta_5 TREAT \times POST + \beta_6 TREAT \times MF\_SURP\_GNEWS\\ &+ \beta_7 TREAT \times MF\_SURP\_BNEWS + \beta_8 POST \times MF\_SURP\_GNEWS\\ &+ \beta_9 POST \times MF\_SURP\_BNEWS\\ &+ \beta_{10} TREAT \times POST \times MF\_SURP\_GNEWS\\ &+ \beta_{11} TREAT \times POST \times MF\_SURP\_BNEWS + \Sigma \Gamma X + \varepsilon \qquad (3.2)\end{aligned}$$

where the set of controls (X) is the same as those discussed above for equation (3.1), but are interacted with *MF_SURP_GNEWS* and *MF_SURP_BNEWS* instead of *MF_SURP*.

[23] Using continuous forms of these variables instead of their binary forms does not qualitatively alter results. See table D.3 in appendix D.

MF_SURP_GNEWS (*MF_SURP_BNEWS*) is equal to *MF_SURP* where *MF_SURP* is positive (negative), and zero otherwise. Skinner (1994) finds a stronger average market reaction to bad news forecasts than to good news forecasts. Accordingly, the effect of fair value accounting on the MFRC may differ for good news and bad news forecasts. Consistent with H1, I predict negative β_{10} and β_{11} coefficients, indicating that treatment firms experience a more negative change in the strength of the reaction to management forecasts from the pre- to the post-period, relative to control firms.

Similar to the unsigned regression (equation (3.1)), I include only those variables that differ statistically between treatment and control groups in each of the good and bad news forecast samples. The results of the t-test of mean differences are reported in table 4.2 and discussed in section 4.2.1.2.

3.2.3 Research Design - Fair Value Accounting and Timeliness of Price Discovery (H2)

To assess how fair value accounting affects the timeliness of price discovery, I compare the change in timeliness of price discovery between derivative users and non-users around SFAS 133. Derivative users (treatment) and non-users (control) are identified as discussed in section 3.2.1. Specifically, I compare the DiD in *IPT* between the portfolios, *DiD_IPT* $(=\{IPT_{treat,post} - IPT_{treat,pre}\} - \{IPT_{control,post} - IPT_{control,pre}\})$. I test *DiD_IPT* using a permutation test, where I create a null distribution of *DiD_IPT* under the assumption that the order of the observed monthly returns does not matter. I then test the statical significance of the observed *DiD_IPT* by comparing it to the null distribution of *DiD_IPT*. I discuss this test in greater detail in section 3.2.3.2.

I use a portfolio-level intraperiod timeliness metric, *IPT*, described in 3.2.3.1, to capture the timeliness of price discovery. Using a portfolio-level IPT metric, as opposed to a firm-level

IPT metric, is crucial for making inferences about the price discovery process. As I discuss in section 3.2.3.1 and explore in appendix B, a portfolio-level metric averages away random news arrival at the firm level and substantially reduces the influence of intraperiod return reversals that can inflate *IPT* values – both of which can render the metric useless for interpreting the timeliness of price discovery.

I also separately examine the subsamples of positive and negative intraperiod return observations. Positive (Negative) intraperiod return observations are those with positive (negative) 12-month abnormal buy-and-hold returns. As discussed in section 3.2.2, the confirmability of financial reports may be more important for management-issued good news disclosures, which generally suffer greater credibility concerns. Since management-issued news is a non-trivial part of the intraperiod news, the impact of fair value exposure on IPT may be asymmetric for positive or negative intraperiod return observations.

Similar to H1, I believe the major confound in the analysis is operational uncertainty. As discussed in section 3.2.2, derivative users can have greater or lower operational uncertainty depending on the derivatives' use and effectiveness, if used as hedges. Operational uncertainty can be associated with greater information asymmetry, which can delay the incorporation of information in prices. Another potential confounding construct is the richness of the information environment, which I proxy using firm size (*MVE*). In the unmatched H2 sample, I find that derivative users are generally larger firms than derivative non-users (see table 4.17, panel A in section 4.3.1.2). Freeman (1987) finds that larger firms reflect earnings information earlier in the fiscal year than smaller firms. Also, Lo and MacKinlay (1990) find that the returns of larger stocks lead those of smaller stocks. The general consensus is that larger firms have a richer information environment as there are greater incentives for private information search/communication. Hence, there may be a spurious correlation between derivative users and IPT because derivative users are generally larger firms.

I control for such confounds by matching derivative users to non-users using one-to-one CEM, similar to H1, as discussed in section 3.2.2. In addition to matching on *OCFVOL, MVE* and FF12, as in H1*,* I also control for the sign of the 12-month buy-and-hold returns. As discussed above, the impact of the confirmatory role of financial reports on IPT may differ for positive and negative intraperiod return observations. By default, I also match on fiscal year as the H2 sample only includes one pre-period year, 1999. I first sort (coarsen) the continuous variables, *OCFVOL* and *MVE,* into bins using the lowest number of cutpoints to achieve covariate balance for each variable. I then perform an exact match on the *OCFVOL* bin, *MVE* bin, FF12 and the sign of the 12-month buy-and-hold returns. Similar to H1, I match each derivative user to a random derivative non-user in the same stratum, without replacement.

3.2.3.1 Portfolio-Level Intraperiod Timeliness Metric

IPT is calculated as the area under a curve representing cumulative buy-and-hold returns over a period, expressed as a percentage of the total 12-month buy-and-hold return. A bigger area indicates more timely incorporation of information in prices. Prior literature has used both portfolio-level IPT metrics, as discussed below, and firm-level *IPT* metrics, as discussed in appendix B, to assess the timeliness of price discovery. In this thesis, I use a portfolio-level metric for two reasons. *First*, a portfolio-level metric reduces the impact of idiosyncratic timing of news arrival inherent at the firm level. The *IPT* metric captures the speed with which all available information within a given period is impounded into prices. More specifically, it is a function of both the timing of news arrival within a given period (news arrival process) and the speed with which this news is communicated and incorporated into stock prices by market participants (price discovery process). Bushman, Smith and Wittenberg-Moerman (2010, p. 931) argue that "[t]he use of portfolio-level analysis instead of regression analysis is crucial to average away the random news arrivals that render firm-period measures extremely noisy." Their goal, and mine, is to make inferences about the price discovery process as opposed to the news arrival process. *Second*, I find that firm-level stock returns often include large reversals within a given period. In such cases, *IPT* values can be artificially inflated and, thus, difficult to interpret. Appendix B provides a discussion of the limitations related to using a firm-level *IPT* metric.

Capital markets research since Ball and Brown (1968) has examined such cumulative abnormal return curves, although not always in the same form as in this thesis. Examples of such studies include Freeman (1987), Alford, Leftwich and Zmijewski (1993), Butler, Kraft and Weiss (2007), and Bushman et al. (2010). I briefly discuss the use of cumulative abnormal return curves for each of these studies. Ball and Brown (1968) plot the abnormal returns separately for positive and negative income forecast error portfolios to show that the market has reacted in the same direction as the sign of the income forecast error. The authors cite this as evidence that the annual accounting income figure has meaning, contrary to prominent beliefs at that time. They also indicate that "the annual income report does not rate highly as a timely medium," since most of its information content is anticipated by the market beforehand. Freeman (1987) tests whether abnormal stock returns of larger firms anticipate accounting earnings earlier than those of smaller firms, by plotting the cumulative abnormal returns for portfolios of large and small firms. Alford et al. (1993) plot the abnormal cumulative returns as a percentage of the 15-month cumulative returns (ending three months after the fiscal year-end) to compare the timeliness of accounting earnings information across countries. Butler et al. (2007) develop an intraperiod timeliness metric that estimates the area under the intraperiod timeliness curve to examine whether financial reporting frequency affects the timeliness of earning information. Bushman et al. (2010) then use a modified version of the IPT metric developed by Butler et al. (2007) to assess whether earlier access to private information via loan syndicates affects the timeliness of price discovery. Bushman et al. (2010) use portfolio-level returns instead of firm-level returns to average away the idiosyncratic news arrival evident at the firm level.

I calculate the portfolio-level *IPT* following Bushman et al. (2010). Using monthly portfolio buy-and-hold cumulative returns over a 12-month period, the formula is:

$$IPT = \frac{1}{2}\sum_{m=1}^{12}(BH_{m-1} + BH_m)/BH_{12} = \sum_{m=1}^{11}(BH_m/BH_{12}) + 0.5 \tag{3.3}$$

where *BH* equals abnormal portfolio buy-and-hold return, and *m* denotes the month. Using this formula, if no news were incorporated until month 12, *IPT* would equal 0.5. Conversely, if all news were incorporated at the beginning of the 12-month period, *IPT* would equal 11.5. Lastly, if news were incorporated evenly throughout the 12-month period, *IPT* would equal 6.

For each of the treatment and control portfolios, I construct portfolio buy-and-hold abnormal returns as the equally weighted hedge return one would earn based on perfect foresight of the 12-month return. Specifically, it is calculated as the return one would earn by taking a long position in firms with a positive 12-month buy-and hold return and a short position in firms with a negative 12-month buy-and hold return. This ensures that the positive and negative returns do not simply cancel out and yield near-zero aggregate returns. I use the sign of the 12-month buy- and hold returns, rather than the change in net income, to create the hedge portfolio returns because I am interested in the timeliness of all price-relevant information, which encompasses but is not exclusive to earnings. For example, changes that flow through comprehensive income may also be relevant to stock prices. It can also include qualitative information in voluntary disclosures that can be confirmed, to some degree, by accounting information. In contrast, prior studies that use equally-weighted hedge portfolio returns based on perfect foresight of the *change in net income* (e.g., Ball and Brown 1968; Alford et al. 1993; Butler et al. 2007) are interested in examining the timeliness with which accounting *earnings* information is incorporated into prices.

3.2.3.2 Test of Difference-in-Differences in Intraperiod Timeliness

I test *DiD_IPT* using a modified version of the permutation test used by Butler et al. (2007), originally developed by McNichols (1984). The null distribution of *DiD_IPT* assumes that the order of the observed monthly return quartets (pre-period treatment, post-period treatment, pre-period control and post-period control) is random and thus, each has an equal probability of representing any given month in the test period. To create the distribution under the null hypothesis, I randomly assign each of the 12 portfolio monthly return quartets to a month, 1 to 12. Using the randomly assigned monthly returns, I calculate *IPT* for each of the four portfolios using the formula in equation (3.3) above. Then, I estimate the *DiD_IPT* as indicated in the formula above. I repeat this process 1000 times to produce a sampling distribution of *DiD_IPT*. I use this sampling distribution to assess the likelihood that the sample statistic, $\widehat{DiD\_IPT}$, will be observed in this null distribution. To aid in interpreting the DiD results, I also examine the statistical significance of *D_IPT*, the change in IPT from the pre- to the post-period, for each of the control and treatment portfolios.

This permutation test differs from that of Butler et al. (2007) in the following ways. First, Butler et al. (2007) create a test statistic, following McNichols (1984), that captures the *largest* *distance* between two *IPT* curves, rather than the *difference in the areas* under each curve. Their implicit assumption is that the largest distance between the two *IPT* curves is increasing in the difference in the areas under the two curves. While this assumption is intuitive, it is possible that the *IPT* curves intersect each other, such that curve A is below curve B for part of the 12-month period and then rises above curve B for the remainder of the period. In such cases, either curve could have the larger area. Hence, I posit that it is more accurate and direct to compare the difference in the areas under *IPT* curves, than to infer this difference from the largest distance between the curves. Second, while Butler et al. (2007) only test the difference between two IPT curves, I adapt their test to assess the DiD between four IPT curves. Consistent with H2, I predict that *ΔIPT* will be more negative (or less positive) for the treatment group, relative to the control group. In other words, I predict that *DiD_IPT* will be negative, indicating that an increase in exposure to fair value accounting decreases the timeliness of price discovery.

3.3 Sample Selection

For all tests, I examine U.S. firms to focus on a single set of accounting standards in a single country with a large amount of data available. The focus on the U.S. aligns with much of the prior literature on fair values. I obtain stock returns data from the Center for Research in Security Prices (CRSP) and financial data from Compustat. All management and analyst forecast data are from Thomson Reuters' Institutional Brokers Estimate System (IBES). I identify derivative users and non-users using a keyword search via SeekEdgar and manual examination of the 10-K filing, as discussed in section 3.2.1. I separately discuss the sample selection processes for H1 and H2 in the following subsections, as H1 is crucially limited by the availability of management forecasts, whereas H2 is not.

3.3.1 Sample Selection – Fair Value Accounting and Credibility of Voluntary Disclosures (H1)

Table 3.1 summarizes my sample selection process for H1. Panel A describes the sample selection process to arrive at the set of H1 observations with necessary management forecast and firm-level data. Then, in panel B, I describe the sample selection process to identify the unmatched and matched H1 samples. Specifically, this panel restricts the sample based on the availability of pre- and post-period observations necessary for a DiD analysis, prior to identifying firms as treatment or control. I delay the identification of treatment and control firms to this later point in the sample identification to minimize hand collection costs. Recall that the identification of treatment and control firms involves keyword search results as well as manual examination of the 10-K filing for some instances. Finally, in panel C, I report the number of treatment and control observations in each of the unmatched and matched H1 samples.

In panel A of table 3.1, I begin with the intersection of U.S. firms on CRSP and Compustat for the pre- and post-SFAS 133 years examined (fiscal 1998, 1999, 2001 and 2002), as discussed in section 3.2. The initial population contains 26,455 firm-year observations for 8,883 unique firms. I exclude financial firms because these firms often hold derivatives for trading or speculative purposes, which were already reported at fair value prior to SFAS 133. This removes 6,951 firm-year observations for 2,221 firms. I then exclude firms with no annual EPS management forecasts (MF) on IBES. The resulting subset comprises 8,296 annual EPS MFs, representing 3,426 firm-years and 1,918 firms. Hence, the data requirement to have EPS MFs on IBES removes more than 80 percent of the population. Nevertheless, it is a necessary data restriction as H1 focuses on EPS MFs.

Next, I apply the following restrictions to my sample based on prior literature (e.g., Rogers and Stocken 2005; Josefy, Rees and Tse 2015). I remove 681 MFs other than point or closed range MFs, since investors face significant ambiguity in comparing and confirming open range or qualitative forecasts against the actual outcome on financial reports. I exclude 412 MFs issued on or after the fiscal-year end forecasted, since these are, in substance, earnings pre-announcements. I also exclude 1,084 MFs issued before the prior year's earnings announcement to ensure that the voluntary disclosure relates primarily to the forecast of current year earnings.

Next, I restrict my sample to observations with sufficient data to calculate the dependent and independent variables of interest. First, I exclude 70 MF observations with insufficient data to calculate $MF\_CAR_{0,1}$, the dependent variable. Next, to construct *MF_SURP*, I exclude 207 MFs with no viable analyst forecast for the corresponding firm-year issued prior to the MF date. In addition, I require the observations to have a corresponding analyst forecast to be issued within the 90-calendar-day window prior to the MF to better isolate new information in *MF_SURP*, as discussed in section 3.2.2. This restriction removes 656 MFs.

Next, I exclude 11 MF observations with a pre-MF share price below $1.00 to avoid the small denominator problem, as MF surprise is divided by the pre-MF share price to construct *MF_SURP*. I exclude 69 extreme MF observations where the absolute value of *MF_SURP* is greater than 0.10, or 10% of price, to ensure results are not biased by extreme values, following Josefy et al. (2015).[24] Next, I exclude 119 MF observations for 70 firm-years with incomplete data to construct control variables. Finally, to avoid having multiple MFs for a given firm-year, I keep

[24] The validity of some extreme *MF_SURP* values is questionable. I trace 10 randomly selected observations where *MF_SURP* is larger than 0.10, and I find that in 7 out of the 10 observations, a data error led to inflated *MF_SURP*.

only the latest MF issued for each firm-year, subject to the above data restrictions, resulting in 2,254 MF observations for 1,325 firms.[25]

The first few lines of table 3.1, panel B isolate the firms with data in both the pre- and the post-periods, for the DiD analysis. The pre-period requirement excludes 1,175 firm-years for 832 firms. The number of MFs in the IBES database is rather small in the earlier years of my sample period; it increases substantially in the early 2000s, as I discuss in section 3.3.1.1 (table 3.3). The post-period data requirement removes 225 firm-years for 199 firms, leaving 854 firm-year observations for 294 firms with at least 1 pre- and 1 post-period observation. I exclude 26 firm-years for 10 firms, for which I cannot classify as treatment (derivative user) or control firms (derivative non-users) due to missing 10-K filing on SEC's EDGAR database. This results in an unmatched H1 sample of 828 firm-years representing 284 firms.

As discussed in section 3.2.2, I also use a matched sample of derivative users and non-users to better control for operational uncertainty. I use CEM, and match on bins of *OCFVOL* (i.e., coarsened *OCFVOL*), *MVE*, FF12 industry and fiscal year. The algorithm selects the minimum number of cutpoints to achieve *OCFVOL* and *MVE* balance between the treatment and control groups. This results in 5 equally spaced cutpoints (i.e., 4 bins) for *OCFVOL* and 4 cutpoints (i.e., 3 bins) for *MVE*, and yields a matched sample of 318 firm-year observations for 112 firms. Table 3.2 provides the results of the t-test of covariate means between treatment and control groups before and after matching, in the latest pre-period year, the year of the match. In panel A, the

[25] I examine the latest MF for a given firm-year as later forecasts are generally more credible than earlier forecasts. For example, Baginski and Hassell (1997) argue that a longer forecast horizon increases earnings uncertainty. Since this thesis examines a construct, fair value exposure, that *reduces*, rather than increases, voluntary disclosure credibility, I focus on MFs that are otherwise more credible. In section 4.2.3.4, I also examine results using the earliest MF of each year that meets the data requirements, to assess the sensitivity of results to forecast horizon. Results are weaker when I use the earliest MF of each year than when I use the latest MF, as suspected.

treatment group has smaller *OCFVOL* and greater *MVE* than the control group. However, after matching, in panel B, *OCFVOL* and *MVE* are no longer statistically different between these two groups, indicating a successful match.

Finally, in panel C of table 3.1, I report the unmatched and matched H1 samples, by treatment (derivative users) and control (derivative non-users) groups, respectively. The unmatched sample comprises 263 firm-year observations for 97 control firms and 565 firm-year observations for 187 treatment firms. Hence, the unmatched sample has more treatment firms than control firms. After applying CEM, the matched sample includes 56 matched pairs of control and treatment firms. It comprises 153 firm-years for the control group and 165 firm-years for the treatment group. The number of firm-year observations in the treatment group does not correspond exactly to that in the control group because I require at least one pre- and one post-period observation, but allow up to two observations for each period for a given firm to expand the sample size. Hence, each firm has a minimum of 2 and a maximum of 4 firm-year observations in the sample. The important matched sample characteristics are that each firm has at least one pre- and one-post period observation and that treatment and control firms are matched in the same pre-period year, to allow for a DiD analysis.

3.3.1.1 H1 Sample Representativeness

The H1 sample restriction related to managers' decision to issue management forecasts and the availability of those forecasts on IBES significantly reduces the sample from the intersection of non-financial U.S. firms on Compustat and CRSP (hereafter, Compustat population). Specifically, as shown in panel A of table 3.1, of 19,504 (26,455 – 6,951) firm-years in the Compustat population, only 3,426 firm-years (17.6%) have management forecasts on IBES (hereafter, annual EPS MF issuers). Given this large reduction as well as further reductions related to characteristics of the management forecasts discussed in 3.3.1 and reported in table 3.1, I assess the representativeness of the unmatched H1 sample (828 firm-years) and the matched H1 sample (318 firm-years). Specifically, I compare the frequency distributions, across years and across industries, between the matched sample, the unmatched sample, set of annual EPS MF issuers, and the Compustat population.

Panels A and B of table 3.3 report the temporal and industry distributions, respectively, of each of the matched and unmatched samples, the set of annual EPS MF issuers, and the Compustat Population. In panel A, fiscal year t includes fiscal years ending June t to May t+1, to correspond to the effective date of SFAS 133, as discussed in section 3.2. I find that the numbers of firms issuing annual EPS MFs is very low in fiscal years 1998 and 1999, relative to those in fiscal years 2001 and 2002, and relative to the Compustat population. The number of annual EPS MF issuers increases drastically beginning in 2001, which is consistent with the increase in voluntary disclosures associated with Reg FD (2000) documented in prior literature (e.g., Anilowski et al. 2007; Choi et al. 2010). Hence, the propensity to issue EPS MFs appears to have increased during my sample period. Using a DiD research design, where I compare treatment firms to control firms over the same period helps to alleviate concerns that changes in MFRC are driven by temporal changes in disclosure behavior.

In both the unmatched and matched samples, however, the earlier years represent a larger proportion of the samples, relative to the proportion represented in the set of annual EPS MF issuers. This is by design, because the restriction to have non-missing pre- and post-period observations removes many firms with only post-period MF observations. Given this restriction, the results in this thesis may not generalize to new firms or firms that just begin to issue MFs around the year 2000.

With respect to industry distribution (Table 3.3, panel B), I find some notable differences between the Compustat population and the unmatched sample. First, the business equipment industry is under-represented (i.e., greater than 3 percentage decrease) in the unmatched sample, relative to the Compustat population. This industry represents 25.5% of the Compustat population, but only 13.6% of the unmatched sample. This large difference is attributable to fewer proportion of EPS MF issuing firms in this industry, noted by the decrease in industry representation from 25.5% in the Compustat population to 20.6% in the set of annual EPS MF issuers, and to fewer firms meeting data requirements in both the pre- and post-periods. Second, the following industries are over-represented (i.e., greater than 3 percentage increase) in the unmatched sample, relative to the Compustat population: consumer non-durables, utilities and wholesale and retail. These differences in representation are partially driven by a larger proportion of EPS MF issuers in these industries. These differences suggest that managers in certain industries are more likely to issue forecasts, though differences could also be caused by differences in completeness of the IBES Guidance data, as I discuss below. Forms of communication may also differ across industries.

Next, I compare the industry distribution of the unmatched sample to the matched sample. The wholesale and retail, and other industries constitute a larger proportion of the matched sample, relative to the unmatched sample, because these industries have more balanced distributions of derivative users and non-users, relative to other industries (see table 4.1). A more balanced distribution of derivative users and non-users allows for a higher likelihood of successful matches, ceteris paribus. In contrast, the manufacturing, utilities and healthcare industries constitute lower proportions of the matched sample than of the unmatched sample. These industries have more derivative users than non-users (see table 4.1), resulting in a lower proportion of matched pairs.

Despite some notable differences in the industry distribution of the H1 samples and the Compustat population, large industries in the Compustat population are generally well represented in the samples. Further, examining an industry matched sample of derivative users and non-users and including multiple industries in the analysis helps to alleviate concerns that results are driven by industry-specific characteristics.

In addition to the differences noted above, I also caveat that the H1 sample may not be representative of the Compustat population due to IBES data limitations. Chuk, Matsumoto and Miller (2013) find that the coverage of management forecasts on Thomson First Call's Company Issued Guidance (CIG) database, the precursor to the IBES Guidance data, is far from complete.[26] In particular, they find that the CIG database is more likely to cover firms with greater analyst following, greater institutional ownership and better firm performance. Accordingly, the H1 results may not generalize to firms with less analyst following, lower institutional ownership or poorer firm performance. I, however, do not expect such coverage issues to confound tests of H1. Although I don't directly control for analyst following and institutional ownership, I eliminate any size differences between derivative users and non-users using the matched sample and prior literature documents that both analyst following and institutional ownership are increasing in firm size.[27] Finally, I control for firm performance by including a control variable for loss forecasts.[28]

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[26] Chuk et al. (2013) examine a random sample of 1,756 management forecasts for the years 1997, 1999, 2001, 2003, 2005 and 2007, manually identified from Lexis Nexis, and find that the CIG database only covers approximately 51% of these management forecasts.

[27] For example, Bhushan (1989) and Shores (1990) note that analysts are more likely to follow larger firms. Similarly, O'Brien and Bhushan (1990) and Duggal and Millar (1999) argue that institutional investors often invest in larger firms for liquidity reasons.

[28] Chuk et al. (2013) use the frequency of losses in the prior eight quarters as a proxy for firm performance. While I do not examine this frequency, I posit that it is positively correlated with the expected likelihood of annual loss reflected in loss forecasts.

3.3.2 Sample Selection - Fair Value Accounting and Timeliness of Price Discovery (H2)

Table 3.4 provides the sample selection process for H2. Panel A reports the sample selection to arrive at the set of H2 observations with necessary IPT data. To test H2, I begin with the intersection of U.S. firms on Compustat and CRSP for fiscal 1999 and 2001, which comprises 13,289 firm-years for 7,765 firms. I exclude 3,497 firm-years for 1,953 firms in the financial industry, for the same reasons as for H1, discussed in section 3.3.1. I then exclude observations with fewer than 10 monthly returns in the 12-month period, to ensure sufficient data to calculate *IPT*. This removes 1,166 firm-years for 601 firms, leaving 8,626 firm-year observations for 5,211 firms with necessary data to calculate *IPT*.

Next, I restrict the sample to firms that have *both* pre- and post-period observations, as shown in panel B of table 3.4. This removes 692 and 1,104 firms with no pre-period observation and post-period observation, respectively, resulting in a set of 6,830 firm-years for 3,415 firms. From this set, I exclude firms that do not have sufficient data to construct the confounding variables, *MVE* and *OCFVOL*, in the pre-period. This removes 306 firm-year observations for153 firms.

Prior to identifying derivative users (treatment) and non-users (control), I remove 3,712 firm-year observations for 1,856 firms with differing signs of intraperiod news (i.e., 12-month buy-and-hold returns) in the pre- and post-periods. As discussed in section 3.2.3, the impact of fair value exposure on IPT may be contingent on whether the intraperiod news is net positive or net negative. To attribute results to fair value exposure and not a change in the sign of intraperiod news from the pre- to the post-period, I restrict attention to firms whose annual returns have the same sign in the pre- and post-periods. I apply this restriction *prior* to identifying treatment firms to minimize data collection costs. Recall from section 3.2.1 that the data collection process for identifying derivative users and non-users involves a combination of SeekEdgar keywords search and hand collection. Next, I exclude 58 firm-years for 29 firms that I am unable to classify as derivative users or non-users due to missing 10-k filings on the SEC's EDGAR database. This results in 2,754 firm-year observations for 1,377 firms in the unmatched sample.

As discussed in section 3.2.3, I use CEM to match on *OCFVOL*, *MVE*, FF12 industry, and sign of intraperiod news, without replacement. For continuous variables, I identify the minimum number of cutpoints to achieve covariate balance between the treatment and control groups, through trial and error. This results in 28 equally spaced cutpoints (i.e., 27 bins) for *OCFVOL*, and 8 cutpoints (i.e., 7 bins) for *MVE*. The CEM matching process drops 1,578 firm-years for 789 firms. Thus, the matched sample comprises 1,176 firm-years for 588 firms.[29]

Panel C provides a breakdown of the H2 samples, by treatment and control groups. The unmatched sample comprises 1,770 firm-years for 885 control firms and 984 firm-years for 492 treatment firms. Hence, prior to matching, the sample includes more control firms than treatment firms. In the matched sample, each of the treatment and control groups comprise 588 firm-year observations for 294 firms. Within the matched sample, 66 pairs of treatment and control firms have positive intraperiod returns and 228 pairs have negative intraperiod returns in both the pre- and post-periods. Hence, each of the treatment and control groups comprise 132 (456) firm-year observations for 66 (228) firms for the positive (negative) intraperiod return subsample.

3.3.2.1 H2 Sample Representativeness

[29] To increase sample size, I create two alternative matched samples, where I use CEM to match on each of *OCFVOL* and *MVE*, separately, in addition to industry and sign of intraperiod news. These results are reported in section 4.3.3.1 as additional analysis.

To gain a better understanding of how the sample selection affects the sample composition, I compare the industry distributions as well as firm characteristics between the non-financial-industry Compustat population (hereafter, Compustat population) and the unmatched and matched H2 samples for each of the pre- and post-periods. Panel A of table 3.5 reports the industry distributions of the Compustat population and the H2 samples. I highlight any differences in industry representations greater than 3 percentage points.

I find that the manufacturing industry is over-represented in the both the unmatched and matched samples relative to the Compustat population, while the business equipment industry is under-represented. In addition, the "other" industry is over-represented and the healthcare industry is under-represented in the matched sample, relative to the 2001 Compustat population. In particular, the business equipment industry comprises 26% and 27.1% of the Compustat population in the 1999 and 2001, respectively, but only 16.7% of the matched sample. A larger proportionate loss in observations in this industry, relative to other industries, is due a large imbalance in the numbers of potential treatment and control firms, prior to matching (see table 4.16, panel A). In contrast, the manufacturing industry experiences a small proportionate loss in observations in the matched sample due to more balanced proportions of potential control and treatment firms (see table 4.16, panel A).

Overall, I observe some substantial differences in the industry distribution between the H2 samples and the Compustat population. Nevertheless, all large industries in the Compustat population continue to be reasonably represented in both the unmatched and matched samples. In addition, using an industry-matched sample of derivative users and non-users across several industries helps alleviate concerns that results are attributable to industry-specific traits.

Panel B of table 3.5 reports the means of descriptive variables for the Compustat population and the unmatched and matched H2 samples. For each variable, I test whether each of the sample means equals the value of the Compustat population mean. I do not perform a two-sample t-test between the Compustat population and the respective sample because the latter is a subsample of the former. Instead, I perform a one sample t-test to examine whether the respective sample mean is equal to the value of Compustat population mean, which does not consider the population's standard deviation or sample size.

In the pre-period, I find that operational cash flow volatility is significantly lower in both the matched and unmatched samples than the mean value of the Compustat population. In addition, I find that both the matched and unmatched samples comprise larger firms with more frequent management forecasts, relative to the Compustat population averages. However, analyst following is larger than the Compustat population mean in only the matched sample. In the matched sample, some of these differences disappear in the post-period. While the matched sample still has lower operational cash flow volatility, these sample firms are not larger and do not have greater analyst following or more frequent management forecasts than the Compustat population in the post-period. Similar to the matched sample, the unmatched sample continues to have lower operational cash flow volatility than the Compustat population. However, unlike the matched sample and unlike the pre-period, in the post-period the unmatched sample has smaller firms, with lower analyst following and less frequent management forecasts than the Compustat population.

Overall, the selection process biases the matched sample in favor of firms with less operational volatility and a more rich information environment, particularly in the pre-period. Accordingly, I caveat that the matched sample results may not generalize to young firms, which may be more volatile and have a less rich information environment. Similar observations pertain to the unmatched sample in the pre-period. However, in the post-period, the unmatched sample firms appear to have a less, not more, rich information environment than the Compustat population.

Chapter 4

Empirical Analyses

4.1 Introduction

This chapter presents the results of the empirical analyses testing the impact of fair value accounting on the credibility of voluntary disclosures (H1) and the timeliness of price discovery (H2). Due to specific characteristics of the intraperiod timeliness metric used as a proxy for timeliness of price discovery, as discussed in 3.2.3.1, H2 is tested at the portfolio level, while H1 is tested at the firm-year level. Furthermore, H1 is crucially limited by the availability of management forecasts, while H2 is not. Given that these differences lead to different research designs and samples, as discussed in chapter 3, I separately discuss the results of the empirical analyses of H1 and H2, in sections 4.2 and 4.3, respectively. Section 4.4 concludes the chapter.

4.2 Fair Value Accounting and Credibility of Voluntary Disclosures (H1)

This section presents the empirical analyses of the impact of fair value accounting on the credibility of voluntary disclosures. Section 4.2.1 discusses the industry distribution and the descriptive statistics of management forecast and firm characteristic variables for the unmatched and matched samples. Section 4.2.2 presents the results of the main analyses, which shows that an increase in exposure to fair value accounting reduces the credibility of good news voluntary disclosures, but not of bad news voluntary disclosures. Section 4.2.3 presents the results of additional analyses that test the sensitivity of my findings to including firm fixed effects, using an alternative matched sample, using only stand-alone forecasts, using the earliest management forecast for each firm-year instead of the latest, and using an alternative set of control variables. I also test the sensitivity of my findings to alternative specifications of the regression model and using a three-day, instead of a two-day, management forecast return. I find that the results of the main analyses, in section 4.2.2, are generally robust to various model specifications and alternative samples and subsamples.

4.2.1 Descriptive Statistics

4.2.1.1 Industry Distribution

Table 4.1 reports the industry distribution of observations in the unmatched and matched H1 samples, using unique firm observations. The manufacturing, business equipment, wholesale and retail and other industries have the largest representations in the unmatched sample. However, the manufacturing industry is no longer one of the largest industries in the matched sample because the large imbalance in derivative users and non-users results in few matches. As discussed in section 3.3.1.1, the proportion of derivative users and non-users in each industry affects the matched sample because I match firms within industry. Meanwhile, the consumer durables, energy and extraction and telecommunications industries have the smallest representations in both the unmatched and matched samples – each industry comprises 10 (4) or fewer firms in the unmatched (matched) sample. In the matched sample, the chemicals and allied products industry also has only 4 firms. Finally, the utilities industry, with only 1 control firm in the unmatched sample, drops out of the matched sample.

I examine the proportion of treatment firms within each industry to assess derivative usage. I observe substantial differences in the proportion of derivative users across industries, ranging from a low of 28.6% in the telecommunications industry to a high of 93.8% in the utilities industry. Prior to matching, treatment firms comprise more than 80% of the manufacturing, chemicals and allied products and utilities industries, indicating large imbalances in the proportion of derivative users and non-users. Treatment firms comprise 50% or less of the following three industries: telecommunications, wholesale and retail and other. Accordingly, industry factors appear to, at least partially, drive the firm's decision to use or not use derivatives. As discussed in section 3.2.2, I match treatment and control firms within each FF12 industry classification, which controls for industry-specific incentives to use derivatives.[30]

Nearly two-thirds of the firms in the unmatched H1 sample are derivative users. However, this proportion of derivative users is not representative of the wider population of firms. In the H2 sample, which is not subject to management forecast data constraints, derivative users comprise only about one-third of the unmatched H2 sample (table 4.16). The unmatched H1 sample has a greater proportion of derivative users than the unmatched H2 sample because firms with annual EPS MFs in IBES are more likely to be derivative users than derivative non-users. This is not surprising because MF issuers tend to be larger and prior literature (e.g., Zhang 2009; Donohoe 2015) documents that derivative users are, on average, larger than derivative non-users. For example, in the Compustat population of non-financial firms in the H1 sample period (fiscal 1998, 1999, 2001 and 2002), the mean *MVE* for *non*-annual EPS MF issuers is $1.388 billion, whereas it is $5.541 billion for annual EPS MF issuers (untabulated).

4.2.1.2 Descriptive Statistics

Table 4.2 reports descriptive statistics of management forecast and firm characteristic variables for the unmatched and matched H1 samples, in panels A and B, respectively. The descriptive statistics of management forecast variables are comparable to those reported in prior literature (e.g., Baginski and Hassell 1997; Ajinkya et al. 2005; Rogers and Stocken 2005; Choi et al. 2010; Cheng,

[30] I find that adding industry fixed effects and their interactions with the management forecast surprise variables in the regression model does not alter results (see appendix D, table D.2).

Luo and Yue 2013; Baginski et al. 2015; Li and Zhang 2015). I find that a little over half of the management forecasts are bad news forecasts, whose EPS is below analyst expectations (*MF_BNEWS*). Similarly, approximately 54% of forecasts have negative management forecast announcement return ($MF\_CAR_{0,1}$) (untabulated). Less than 2% of management forecasts forecast losses. Approximately 38% of forecasts forecast a specific target EPS, as opposed to a range (*MF_WIDTH* equal to 0) (untabulated). Also, a little over half of the management forecasts are issued concurrently with an earnings announcement (*EA_CONCUR*) (hereafter, bundled forecasts).[31]

Panels C.1 to D.3 report the descriptive statistics of management forecast and firm characteristic variables, by control and treatment firms, as well as the results of the t-test of means for the unmatched and matched samples/subsamples. Panels C.1, C.2 and C.3 report the descriptive statistics and the t-test results for the combined, good news (positive *MF_SURP*), and bad news (negative *MF_SURP*) unmatched sample/subsamples, respectively. Similarly, panels D.1, D.2 and D.3 report the descriptive statistics and the t-test results for the combined, good news, and bad news matched sample/subsamples, respectively.

In the combined unmatched sample, I find that treatment firms have more positive forecast surprises (*MF_SURP*) than control firms. Also, treatment firms have smaller cash flow volatility (*OCFVOL*) than control firms, which may be attributable to lower economic volatility through the effective use of hedges. The non-linearity term, *MF_SURP*×|*MF_SURP*|, is more positive in treatment firms than in control firms, similar to *MF_SURP*. Treatment firms are larger (*MVE)* and, thus, may have a richer information environment than control firms. They are also less likely to forecast losses (*MF_LOSS*) and have more precise forecasts (i.e., smaller *MF_WIDTH*) than

[31] I find that excluding these forecasts strengthens the results supporting H1. See section 4.2.3.3.

control firms.

In both the good and bad news subsamples, treatment firms have lower cash flow volatility (*OCFVOL)* and more positive $MF\_SURP \times |MF\_SURP|$ and are larger (*MVE*). In addition, in the good news subsample, treatment firms are less likely to forecast losses (*MF_LOSS*) and, in the bad news subsample, treatment firms have more positive forecast surprises (*MF_SURP*).

In the combined unmatched sample as well as the good and bad news unmatched subsamples, *MF_HORIZON, MTB, EA_CONCUR, EA_SURP,* $EA\_SURP \times |EA\_SURP|$ and *EA_LOSS* are not statistically different between the treatment and control groups. Thus, to limit the number of regressors, I do not include these variables as controls in the tests of H1. Furthermore, I exclude *MF_WIDTH* from the regression conditioned on the sign of news as it does not differ statistically between the treatment and control groups within each of the good and bad news unmatched subsamples. Note each control variable contributes 2 regressors to the regression not conditioned on the sign of management forecast news (hereafter, unconditioned regression) and 3 regressors to the regression conditioned on the sign of news (hereafter, conditioned regression), because I interact each control variable with *MF_SURP* (*MF_SURP_GNEWS* and *MF_SURP_BNEWS*). Also, recall from section 3.2.2, that all continuous variables are transformed into binary variables based on a median split, prior to their inclusion in the regression analyses, to facilitate the interpretation of interaction terms.

In contrast to the unmatched sample, the t-test results of the matched sample, in panel D.1 of table 4.2, show no remaining differences in the observed variables between the treatment and control groups, except for $MF\_SURP \times |MF\_SURP|$. Based on these results, I include this variable as a control in the unconditioned regressions, using the matched sample. In the good news subsample, there are no differences in the observed variables between the treatment and control groups. However, in the bad news subsample, treatment firms are larger (*MVE*), have lower cash flow volatility (*OCFVOL*) and are more likely to have bundled forecasts (*EA_CONCUR*). This imbalance in *OCFVOL* and *MVE,* despite matching on these variables, results because the CEM process provides matches at the firm level using the latest pre-period year, while the t-tests use firm-year observations, consistent with the level of observations used in the regression analyses. The purpose of the CEM process is to identify treatment and control firms that are similar in terms of the potential confounds *prior* to treatment (i.e., SFAS 133). To control for this remaining difference in *OCFVOL* and *MVE*, I include the binary variables, *HiOCFVOL* and *HiMVE,* and their interactions with *MF_SURP_GNEWS* and *MF_SURP_BNEWS* in the conditioned regressions, using the matched sample. I also include *EA_CONCUR* and its interactions with the surprise variables since *EA_CONCUR* also differs significantly between the treatment and control groups in the bad news subsample.

4.2.2 The Effect of Fair Value Accounting on the Credibility of Voluntary Disclosures

This section presents the results of the empirical analysis of H1, which predicts that greater exposure to fair value accounting reduces the credibility of voluntary disclosures. Specifically, I compare the DiD in the MFRC between derivative users and non-users, pre- and post-SFAS 133, as discussed in section 3.2.2.

Panels A and B of table 4.3 present the regression results before and after conditioning on the sign of *MF_SURP*, respectively. In both panels, columns (1) - (3) present the results for the unmatched sample and columns (4) and (5) present the results for the matched sample. Consistent with H1, I predict a negative coefficient on *TREAT×POST×MF_SURP* (*TREAT×POST×MF_SURP_GNEWS, TREAT×POST×MF_SURP_BNEWS*), suggesting that the change in credibility from the pre- to the post-SFAS 133 period is more negative for treatment firms than for control firms. Column (1) of panel A reports the unconditioned regression results using the unmatched sample prior to the inclusion of control variables. I find that the coefficient on *TREAT×POST×MF_SURP* is marginally positive, contrary to H1. The marginally positive coefficient persists after controlling for operational uncertainty, in column (2). However, once we add all relevant control variables, in column (3), *TREAT×POST×MF_SURP* becomes statistically insignificant. I find that the loss in significance is largely attributable to the inclusion of the non-linearity variable (see table D.5, panel A, column (2)). Hence, the failure to control for non-linearity, when it differs significantly between treatment and control groups, can lead to misleading results. In the matched sample, in columns (4) and (5), *TREAT×POST×MF_SURP* is not statistically significant. Thus, the results do not support H1, using the unconditioned regression.

I draw attention to a few noteworthy coefficients. First, *MF_SURP* is positive and statistically significant in all columns, suggesting that management forecasts are credible in the pre-period. In this period, I do not find a different investor response for treatment versus control firms (*TREAT×MF_SURP*). Second, in the matched sample (columns (4) and (5)), I find some evidence indicating that the MFRC is stronger in the post-period (*POST×MF_SURP*), relative to the pre-period. The increase in management forecast credibility might be due to more frequent communication of private information in the post-Reg FD period, evidenced by Heflin et al. (2003). Third, I find a negative coefficient for *HiOCFVOL × MF_SURP*, in column (3), suggesting that firms with greater operational uncertainty have less credible forecasts, as predicted. As discussed in section 3.2.2, it is likely more difficult for both managers and investors to accurately predict earnings when operational uncertainty is high. Rogers and Stocken (2005) document that such forecasting difficulty motivates managers to strategically bias their forecasts, making them less credible. Accordingly, it is important to control for operational uncertainty, especially since it differs significantly between treatment and control groups, as observed in panel C.1 of table 4.2.

The significant coefficients on the remaining control variables have the predicted sign, where a sign is predicted, with the exception of *HiMF_WIDTH*×*MF_SURP*. I predicted a negative coefficient for this interacted variable because Baginski et al. (1993) find that less precise forecasts are expected to induce weaker market reactions, as discussed in section 3.2.2. However, I find a positive and statistically significant coefficient for this variable in column (3), suggesting that less precise forecasts have stronger, not weaker, market reactions, contrary to predictions. To investigate this opposite result, I regress $MF\_CAR_{0,1}$ on *MF_SURP*, an indicator variable that equals one for closed range forecasts, and zero for point forecasts, and the interaction of these two variables, which brings the regression model closer to that of Baginski et al. (1993). However, I continue to find opposite results in this model (untabulated). Accordingly, the differing results may be driven by sample differences, rather than model differences. Specifically, Baginski et al. (1993) study the sample period 1983 to 1986, whereas I examine the period 1998 to 2002. Also, Baginski et al. (1993) include open range forecasts in their sample, whereas I do not. Finally, my sample is subject to other restrictions incremental to those included in Baginski et al.'s (1993) sample, such as exclusion of extreme *MF_SURP* observations or the requirement to have non-missing MF observations in both the pre- and post-periods.

Panel B of Table 4.3 presents the regression results for equation (3.2), allowing the MFRC to differ for good news and bad news forecasts. I first discuss the results for good news forecasts and then move on to discuss the results for bad news forecasts. In the unmatched sample, *TREAT*×*POST*×*MF_SURP_GNEWS* is statistically insignificant both before and after adding control variables (columns (1)-(3)). In the matched sample, prior to including *HiOCFVOL*, *HiMVE* and *EA_CONCUR* and their interactions with forecast surprise in the regression (column (4)), *TREAT*×*POST*× *MF_SURP_GNEWS* is statistically insignificant.[32] Once, I include these control variables in the regression model (column (5)), the coefficient for *TREAT*×*POST*× *MF_SURP_GNEWS* becomes negative and statistically significant at the 5% level (β = -8.445; t-stat = -1.88). This is consistent with H1 and suggests that investors perceive management forecasts of firms with high fair value exposure as less credible when the forecast contains good news.

The gain in significance of the coefficient for *TREAT*×*POST*×*MF_SURP_GNEWS* moving from column (4) to column (5) is not attributable to the inclusion of any single control variable in column (5) (see table D.5, panel B). Rather, the addition of all three controls and their interactions with forecast surprise, in combination, give rise to the negative and significant coefficient for *TREAT*×*POST*×*MF_SURP_GNEWS*. The results using the matched sample are likely more reliable than those of the unmatched sample because using CEM to match on potential confounding variables does not require a linearity assumption, whereas including these variables in the regression model does. While the matched sample regression also includes controls for operational uncertainty and the richness of the information environment, similar to the unmatched sample, this is in *addition* to controlling for these confounds in the pre-period using matching, rather than as a substitute for matching.

Next, I focus on the results for bad news forecasts. The coefficient for *TREAT*×*POST*× *MF_SURP_BNEWS* is statistically insignificant in all columns, suggesting that an increase in exposure to fair value accounting does not affect the credibility of bad news forecasts. Prior literature (e.g., Jennings 1987; Williams 1996; Rogers and Stocken 2005) generally suggests that investors perceive bad news from management to be inherently more credible than good news.

[32] As discussed in section 4.2.1.2, I match derivative users and non-users on *OCFVOL* and *MVE* at the firm level using the latest pre-period values. Hence, these proxies are balanced in the latest pre-period year. However, the t-test of differences in means are performed at the firm-*year* level, synonymous with the regression analysis and, thus, can still result in statistical differences in the matched variables.

Thus, the confirmability of financial reports may have little impact on the credibility of bad news forecasts.

I draw attention to some other variables of interest. First, I find that bad news forecasts are credible in the pre-period (positive and statistically significant *MF_SURP_BNEWS*), but good news forecasts are not (insignificant *MF_SURP_GNEWS*). Hutton, Miller and Skinner (2003) argue that investors are more skeptical about forecasts that convey good news. Josefy et al. (2015) documents this null reaction to good new forecasts in their pre-2000 subsample, which partially corresponds to my pre-period (see panel A of their table 6).

Second, I find some evidence suggesting that, in the pre-period, derivative users had more credible good news forecasts than non-derivative users (positive and significant coefficient on *TREAT×MF_SURP_GNEWS* in column (5)). It is possible that derivative users effectively hedged their risk exposures, reducing their economic volatility. This, in turn, would make it easier to predict earnings and might constrain management from misreporting private information in voluntary disclosures, as documented in Rogers and Stocken (2005). While I control for operational uncertainty using *OCFVOL*, there may be aspects of economic volatility that is not fully captured in *OCFVOL*. In contrast, I find no statistical difference in the credibility of bad news forecasts between derivative users and non-users (*TREAT×MF_SURP_BNEWS*) in the pre-period. Differences in predictability may have little impact on the confirmability of financial reports when managers forecast bad news.

Third, I find that *POST×MF_SURP_GNEWS* is positive and statistically significant, in columns (3) and (4), suggesting an increase in the credibility of good news management forecasts from the pre- to the post-SFAS 133 period, in control firms. This is consistent with my prediction that improvements in firms' information environments, following Reg FD (2000), can enhance the credibility of management forecasts. Similarly, I find some evidence suggesting greater credibility of derivative non-users' bad news forecasts in the post-period than in the pre-period. Specifically, the coefficient for *POST×MF_SURP_BNEWS* is positive and statistically significant in the matched sample (columns (4) and (5)).

Fourth, *HiOCFVOL×MF_SURP_GNEWS* is negative and statistically significant, except for in the matched sample (column (5)), suggesting that firms with greater operational uncertainty have less credible good new forecasts. This is consistent with greater difficulty of forecasting earnings for firms with greater operational uncertainty, which, in turn, exacerbates the credibility of forecasts. In contrast, the coefficients for *HiOCFVOL× MF_SURP_BNEWS* are statistically insignificant in all columns, suggesting that operational uncertainty does not affect the credibility of bad news forecasts.

Finally, the significant coefficients on the remaining control variables have the predicted sign, where a sign is predicted, with the exception of *MF_LOSS× MF_SURP_BNEWS*. I predicted a negative coefficient for this interacted variable because loss forecasts are expected to be less informative, and, thus, induce weaker market reactions, as discussed in section 3.2.2. However, I find a positive and statistically positive coefficient for this variable in column (3), suggesting that loss forecasts have stronger market reactions when the managers' forecasted loss is below prior analyst expectations. It is also possible that investors perceive bad news forecasts as optimistic when managers forecast losses; thus, investors react more negatively to the bad (i.e., negative) news to correct for the perceived optimism, which would result in a positive coefficient. For example, a firm may prefer to release bad news slowly to mitigate extreme market reactions. In contrast, loss forecasts indeed have weaker market reactions when managers' loss forecasts exceed analyst expectations, consistent with predictions.

Overall, these results provide some support for H1 for good news forecasts. However, the results are not consistent across the unmatched and matched samples. In contrast, I do not find any support that an increase in exposure to fair value accounting affects the credibility of bad news forecasts. As discussed in section 3.2.2, given that management-issued bad news is inherently more credible than management-issued good news, it is possible that the confirmability of financial reports is influential for the credibility of good new disclosures, but not for bad news disclosures.

4.2.2.1 Change in the Firm's Decision to Use/Not Use Derivatives

Given the inconsistent results in table 4.3, I assess the validity of my classification of treatment and control firms by examining whether treatment (control) firms continue to use (not use) derivatives in the post-period. Recall that I identify treatment and control firms based on their use or non-use of derivatives in the latest pre-period. However, a firm's decision to use or not use derivatives can change from period to period. If treatment firms discontinue their use of derivatives in the post-period, they are no longer exposed to the fair value accounting under SFAS 133. In contrast, if control firms begin using derivatives in the post-period, they will then be exposed to the fair value accounting under SFAS 133. Hence, control firms that begin using derivatives will experience an increase in exposure to fair value accounting whereas treatment firms that discontinue their use of derivatives will experience no change or a decrease in exposure to fair value accounting, biasing against finding results in support of H1.[33]

To examine any changes in firms' decisions to use (not use) derivatives, I identify whether firms use derivatives in the post-period, following the same procedures as for the pre-period

[33] The change in exposure to fair value accounting for treatment firms that discontinue their use of derivatives depends on the type of derivatives held in the pre-period. Treatment firms that held speculative derivatives will experience a decrease in exposure to fair value accounting since these derivatives were already fair valued prior to SFAS 133, as discussed in section 3.2. In contrast, treatment firms that held only derivatives qualifying for hedge accounting will experience no change in their exposure to fair value accounting.

observations, described in section 3.2. Table 4.4 reports the samples of treatment (control) firms that continue to use (not use) derivatives in the post-period. Panels A and B report on the unmatched and matched H1 samples, respectively. In the unmatched sample (panel A), 9 out of 187 (4.8%) treatment firms stop using derivatives and 44 out of the 97 (45.4%) control firms begin using derivatives between the pre- and post-periods. It is possible that firms that stop using derivatives do so to avoid the burden of more stringent hedging criteria and disclosures under SFAS 133. However, these firms are relatively few and likely comprise firms whose derivative activity was not an important part of their operations.

In contrast, the proportion of control firms using derivatives in the post-period is alarming. Consistent with this observation, prior research (e.g., Abdel-Khalik and Chen 2015; Chang et al. 2016; Campbell et al. 2020) documents an increase in derivative users after SFAS 133. Abdel-Khalik and Chen (2015) argue that hedge accounting incentives under SFAS 133, which can reduce earnings volatility for effective hedgers, motivated a growth in non-trading derivative activities. This highlights self-selection concerns, which can attenuate results. Specifically, it is likely easier to forecast earnings for firms with lower earnings volatility, relative to those with greater earnings volatility. As discussed in 4.2.2, Rogers and Stocken (2005) document that managers are less likely to bias management forecasts when forecasting difficulty is lower, relative to greater, making forecasts more credible. Thus, derivative users that experience a decrease in earnings volatility due to SFAS 133 should experience an increase in management forecast credibility, which biases against the predicted H1 relation.

To remove this bias and satisfy the DiD research design assumption that the classification of treatment and control firms (i.e., derivative users and non-users) is stable throughout the sample period, as discussed in section 3.2, I use constant derivative samples for my tests, in addition to the full samples. Eliminating firms that change their decision to use (not use) derivatives leaves 231 unique firms with at least one post-period observation whose derivative use (non-use) is consistent with the pre-period treatment/control classification (table 4.4, panel A). These firms comprise 674 firm-years. I exclude another seven firm-years (but no firms) because, while these firms have at least one post-period observation consistent with the pre-period classification, derivative use (non-use) in these particular firm-years are inconsistent.[34] Thus, the constant derivative unmatched sample comprises 667 firm-years for 231 unique firms, of which 53 are control firms and 178 are treatment firms. In this sample, all treatment (control) firm-years use (do not use) derivatives, resulting in a sample of constant derivative users/non-users.

I similarly restrict the matched sample (see panel B). Nearly half of the control firms (29 out of 56 firms) begin to use derivatives in the post-period, while only a small number of treatment firms (4 out of 56 firms) discontinue their use. Applying the same sample restrictions as described for the unmatched sample results in a constant derivative matched sample of 219 firm-years for 79 firms. Given the small number of control firms in the matched sample, I caution against interpreting results based solely on the matched sample. Thus, I continue to assess H1 using both the constant derivative unmatched and matched samples.

Prior to re-analyzing H1 using the constant derivative sample, I assess covariate balance between treatment and control groups in the constant derivative matched sample. Because some treatment firms do not have matching control firms and vice versa for the control firms in the constant derivative matched sample, the covariates may no longer be balanced between treatment and control firms. To ensure covariate balance, I perform a t-test of the covariate means between

[34]For example, a firm may have four observations, one for each of the fiscal years 1998, 1999, 2001 and 2002 and this firm may use derivatives in 1998, 1999 and 2001, but not in 2002. In this case, I exclude the 2002 observation from the constant derivative sample.

treatment and control groups using the constant derivative matched sample/subsamples in the latest pre-period, the year of match. Panel A, B and C of table 4.5 report the results of this analysis for the combined, the good news and the bad news constant derivative matched sample/subsamples, respectively. I find no differences across all constant derivative matched sample/subsamples, which provides some assurance that the results of the constant derivative matched sample are still valid for interpreting the impact of exposure to fair value accounting on management forecast credibility.

In sum, in the previous analysis in section 4.2.2 (table 4.3), the control groups in both the unmatched and matched samples include a substantial proportion of firms that became exposed to fair value accounting in the post-period; these are not appropriate control firms. Given the inclusion of affected firms in the control group, one may expect to observe a weak negative coefficient on *POST×MF_SURP* in panel A of table 4.3. However, because Reg FD was also enacted between the pre- and the post-period, this confounds inferences about *POST×MF_SURP*. I posit that Reg FD can enhance the credibility of management forecasts by enhancing the information environment, as discussed in section 3.2.2. The positive and significant coefficient on *POST× MF_SURP* in the matched sample (table 4.3, panel A, columns (4) and (5)) provides support for Reg FD effects. This highlights the importance of using a DiD analysis to help isolate the impact of SFAS 133 from the effects of Reg FD, which affects all firms. Overall, a mis-identification of treatment and control firms can bias against finding results by reducing the DiD in the MFRCs between treatment and control firms. Thus, the null or weak results observed in table 4.3 may be an artifact of this poor classification. To assess whether this is the case, I re-analyze H1 using the constant derivative unmatched and matched samples.

4.2.2.2 Exclude Firms that Change Decision to Use/Not Use Derivatives

Table 4.6 reports the results of testing H1 using the constant derivative samples.[35] For brevity, other than for *TREAT×POST×MF_SURP (TREAT×POST×MF_SURP_GNEWS* and *TREAT×POST× MF_SURP_BNEWS*), I only discuss coefficients that may be of interest and are different from those reported in table 4.3. Panel A provides the unconditioned regression results. Similar to the results in table 4.3, in column (1), I find a positive and marginally significant coefficient on *TREAT×POST×MF_SURP* (β = 3.342; t-stat = 1.64). However, similar to the results in table 4.3, this significance disappears once I add all relevant control variables (column (2)), indicating a lack of support for H1. The results in the constant derivative matched sample (columns (3) and (4)) are also consistent with the findings in table 4.3.

Next, in Panel B of table 4.6, I examine H1, conditioning on the sign of *MF_SURP*. I find that the coefficient on *TREAT×POST×MF_SURP_GNEWS* is negative and statistically significant in the unmatched sample with control variables and in the matched sample (column (2): β = -15.529; t-stat = -2.33; column (3); β = -11.354; t-stat = -1.43; column (4): β = -16.759; t-stat = -1.96). Recall that, in panel B of table 4.3, this coefficient was negative and statistically significant only in the matched sample after additionally controlling for *HiOCFVOL, HiMVE* and *EA_CONCUR*. In column (1), which reports the results for the unmatched sample with no control variables, the coefficient is statistically insignificant. However, given that I observe a negative and marginally significant coefficient on *TREAT×POST×MF_SURP_GNEWS* in the matched sample, prior to additionally controlling for *HiOCFVOL, HiMVE* and *EA_CONCUR*, I posit that the lack of significance in the unmatched sample with no controls is related to the lack of control for important confounds such as operational uncertainty or the richness of the information

[35] In table 4.6, I do not repeat column (3) of table 4.3, which was included to show how including a control for the major confound, operational uncertainty (*OCFVOL*), absent other controls affects the coefficients of interests. For reference, I include this analysis for the constant derivative sample in table D.5 (panel A, column (6), panel B, column (5)). Results are similar to those reported in table 4.3, column (3).

environment. Hence, using the constant derivative samples provides stronger and more consistent results in support of H1 for good news forecasts, relative to using the full samples in table 4.3.

Consistent with the results in table 4.3, I find that derivative users have more credible good news forecast in the pre-period than derivative non-users (positive coefficient for *TREAT×MF_SURP_GNEWS*). However, I find this result not only in the matched sample with additional controls, as in table 4.3, but also in the unmatched sample with controls. Hence, derivative users (non-users) that continue to use (not use) derivatives versus those that stopped (began) using derivatives in the post-period may be systematically different firms to begin with. I also find more consistent results that the credibility of good news management forecasts improved from the pre- to the post-period (positive coefficient for *POST×MF_SURP_GNEWS*), relative to the results in table 4.3.

Next, I focus on the results of bad new forecasts in panel B of table 4.6. Across all columns, the coefficient on *TREAT×POST×MF_SURP_BNEWS* is not statistically significant, consistent with the results in panel B of table 4.3. In contrast to the results in table 4.3, I find some weak evidence that derivative users had less credible bad news forecasts than derivative non-users in the pre-period (negative coefficient for *TREAT×MF_SURP_BNEWS* in column (2)).

Overall, I find stronger results supporting H1 for good news management forecasts in the constant derivative samples than in the full samples. This indicates that the inclusion of firms that use (do not use) derivatives in the post-period in the control (treatment) group weakened results in table 4.3. I continue to find null results for H1 for bad news forecasts. This suggests that while fair value accounting under SFAS 133 reduced the credibility of derivative users' good news management forecasts, it does not affect the credibility of bad news forecasts. As discussed above, investors are generally less skeptical of bad news forecasts. Thus, the confirmability of financial reports may be less important for managers' bad news voluntary disclosures. I caveat that the generalizability of these results may be limited to firms that continue to use/not use derivatives throughout the sample period.

4.2.3 Additional Analysis

In this section, I test the sensitivity of the findings of the constant derivative samples from table 4.6 (hereafter, primary results) to including firm fixed effects, using an alternative matched sample, using only stand-alone forecasts, using the earliest management forecasts for each firm-year instead of the latest, and including an alternative set of control variables. In addition, I test the sensitivity of my findings to alternative specifications of the regression model and using a three-day, instead of a two-day, management forecast return. Recall that the primary results in the regression unconditioned on the sign of *MF_SURP*, reported in panel A of table 4.6, do not support H1. This null result, in this regression, is driven by differing results for good news and bad news forecasts. In the regression conditioned on the sign of *MF_SURP,* reported in panel B of table 4.6, the primary results support H1 for good news forecasts, but not for bad news forecasts. In the additional analyses, I use the constant derivative samples, unless otherwise stated. As discussed in section 4.2.2.1, some of the control and treatment firms change from derivative users to non-users or vice versa in the post-period, which can bias against finding results supporting H1.

4.2.3.1 Firm Fixed Effects

In this sub-section, I analyze H1 after including firm fixed effects to control for unobservable firm-specific characteristics that are relatively constant across time within a given firm. This alleviates concerns that the results are driven by unobservable inherent differences between derivative users and non-users, rather than by an increase in exposure to fair value accounting. Table 4.7 reports the results of this additional analysis of H1, after including firm fixed effects. Panel A reports the unconditioned regression results. In the unmatched sample (column (1)), I continue to find a statistically insignificant coefficient for *TREAT×POST ×MF_SURP,* similar to the primary results reported in panel A of table 4.6. However, in the matched sample (column (2)), the coefficient for *TREAT×POST×MF_SURP* is negative and significant at the 1% level (β = -6.514; t-stat = -3.00), which contrasts with the null results reported in table 4.6. Including firm fixed effects provides results supporting H1 using the matched sample, in the unconditioned regression.

Next, panel B of table 4.7 reports the conditioned regression results. In the unmatched sample (column (1)), I find that the coefficient for *TREAT×POST×MF_SURP_GNEWS* is negative and significant at the 5% level (β = -13.897; t-stat = -1.83). In the matched sample, it is negative and significant at the 10% level (β = -13.804; t-stat = -1.66). The coefficient on *TREAT×POST×MF_SURP_BNEWS* is not statistically significant in either sample, consistent with the primary results, reported in panel B of table 4.6.

In addition, I find that the coefficient for *HiOCFVOL×MF_SURP_BNEWS* is positive and significant at the 10% significance level in the matched sample (column (2)), inconsistent with the primary results. The positive coefficient may indicate that investors perceive bad news forecasts of firms with high operational uncertainty as less credible due to optimism. Recall that a more negative reaction to bad news forecasts would manifest as a positive coefficient in the regression. In summary, the test variables are consistent with that of the primary analysis. Thus, the primary results are not driven by unobservable inherent differences between derivative users and non-users.

4.2.3.2 Alternative Matched Sample

In section 4.2.2.2, I exclude firms that change their decision to use (not use) derivatives, which can confound results. While I find that the covariates are still balanced in the constant derivative matched sample, identified in panel B of table 4.4, this sample does not preserve all matched pairs of control and treatment firms. Recall that only 27 (52) out of the 56 control (treatment) firms in the matched sample continue to not use (use) derivatives in the post-period (table 4.4, panel B). Thus 29 control and 4 treatment firms are dropped from the constant derivative matched sample, resulting in an unbalanced number of treatment and control firms.

To alleviate concerns over an incomplete matched sample, I create an alternative matched sample, where I match control and treatment firms within the constant derivative unmatched sample of 231 firms, from panel A of table 4.4. I use CEM to match on *OCFVOL* (5 cutpoints), *MVE* (4 cutpoints) , industry and year. This matching process yields 28 matched pairs of treatment and control firms, comprising 71 control and 81 treatment firm-year observations, for a total of 152 firm-year observations for 56 unique firms. Panel A of table 4.8 reports the results of the t-test of pre-treatment covariate means between the control and treatment groups in the combined alternative matched sample. Both covariates, *OCFVOL* and *MVE*, are balanced, indicating a successful match.

I also test for differences in management forecast and firm characteristics between the control and treatment groups using firm-year observations to assess which control variables I need to retain in the regressions using the alternative matched sample. Panels B, C and D report these results for the combined, good news and bad news alternative matched sample/subsamples, respectively. In the combined sample (panel B), I find that *OCFVOL* and *MF_HORIZON* are smaller and *MVE* is larger in the treatment group than in the control group. Hence, I control for *HiOCFVOL, HiMF_HORIZON*, and *HiMVE* and their interactions with *MF_SURP* in the unconditioned regression model. In the good news subsample (panel C), I find that *MF_HORIZON* is smaller in the treatment group than in the control group. Finally, in the bad news subsample (panel D), the treatment group has smaller *OCFVOL* and larger *MVE* than the control group. Hence, I retain controls for *HiOCFVOL, HiMF_HORIZON,* and *HiMVE* and their interactions with *MF_SURP_GNEWS* and *MF_SURP_BNEWS* in the conditioned regression model. However, given the small number of observations, I control for each variable plus their interaction terms in separate regressions to prevent overfitting the model. Accordingly, I only rely on results that are consistent across all regressions to ensure that any single result is not driven by the failure to control for a relevant management forecast or firm characteristic.

Table 4.9 reports the results of testing the impact of exposure to fair value accounting on the credibility of management forecasts using the alternative matched sample. In panel A, I find that the coefficient for *TREAT×POST×MF_SURP* is not statistically significant across all columns, consistent with the primary results in panel A of table 4.6. In panel B, I find that the coefficient for *TREAT×POST×MF_SURP_GNEWS* is negative and statistically significant across all three columns (column (1): β = -13.000; t-stat = -1.76; column (2): β = -13.113; t-stat = -1.31; column (3): β = -11.785; t-stat = -1.55), consistent with the primary results in panel B of Table 4.6. However, in contrast to the primary results, the coefficient for *TREAT×POST× MF_SURP_BNEWS* is positive and statistically significant at the 10% level in column (3) (β = 4.959; t-stat = 1.35). However, this result is not robust to controlling for *OCFVOL* (column (1)) and *MF_HORIZON* (column (2)), which are relevant controls, based on the results reported in table 4.8. Hence, I conclude that fair value accounting does not affect the credibility of bad news forecasts. In addition, contrary to the primary results and to predictions, the coefficient on *HiOCFVOL× MF_SURP_GNEWS* is *positive* and statistically significant at the 1% level (column (1)). It is possible that, given the small number of observations in this sample, this unexpected result may be caused by overfitting of the regression model. Hence, results must be interpreted with caution. Overall, the primary results, reported in table 4.6, are not sensitive to an alternative matched sample.

4.2.3.3 Stand-Alone Management Forecasts

Next, I test H1 in the subsample of stand-alone management forecasts. In bundled forecasts, the concurrent earnings announcement can add noise to the test of H1, potentially weakening results. Removing these forecasts isolates the management forecast response coefficient from the concurrent earnings announcement news. Thus, I expect to observe stronger results in support of H1 for the stand-alone forecast subsample than for the constant derivative sample (table 4.4), which includes both stand-alone and bundled forecasts. For this subsample analysis, I do not use the matched sample because excluding stand-alone management forecasts in this sample no longer preserves a pre- and post-period observation for each firm, nor the pair of treatment and control firms, defeating the purpose of using a matched sample. Alternatively, I can create a new matched sample within the subset of constant derivative observations with stand-alone forecasts in both the pre- and post-periods. However, this retains only 9 and 20 unique control and treatment firms, respectively. Hence, after matching, this sample would contain a maximum of 9 pairs of control and treatment firms, which is insufficient for regression analysis. Hence, this analysis is restricted to stand-alone forecasts in the constant derivative *un*matched sample.

Table 4.10 report these results. In the unconditioned regression, in panel A, the coefficient for *TREAT×POST×MF_SURP* is statistically insignificant, consistent with the primary results in panel A of table 4.6. Next, in the conditioned regression, in panel B, I find that the coefficient for *TREAT× POST×MF_SURP_GNEWS* is negative and statistically significant (β = -41.710; t-stat = -4.08), consistent with the primary results in panel B of table 4.6. I highlight that the statistical significance of this coefficient is stronger in the stand-alone subsample (1% significance level) than in the primary results (5% significance level). The coefficient on *TREAT×POST× MF_SURP_BNEWS* continues to be statistically insignificant. Thus, excluding bundled forecast from the analysis produces stronger results in support of H1 for good news forecasts.

4.2.3.4 Earliest Management Forecast for Each Firm-Year

Where multiple management forecasts are available for a given firm-year, I use the latest management forecast (that meet the data requirements) to test H1. As discussed in section 3.3.1, I do this because I expect to observe the greatest effect size using later forecasts. Since fair value exposure is predicted, in this thesis, to *reduce*, rather than increase, forecast credibility, I expected the greatest effect size for forecasts that are otherwise more credible. Baginski and Hassell (1997) argue that shorter horizon forecasts are generally more credible than longer horizon forecasts.

The mean (median) *MF_HORIZON* is 237 (255) days for the unmatched sample and 236 (259) days for the matched sample (untabulated), using the *earliest* management forecast of each firm year.[36] Meanwhile, the mean (median) *MF_HORIZON* is 136 (98) days for the unmatched sample and 141 (101) days for the matched sample (untabulated), using the *latest* management forecast of each firm year. Hence, the earliest forecasts' mean (median) *MF_HORIZON* is approximately 3-4 months (4–5 months) longer than that of the latest forecasts. The mean (median) absolute size of the forecast surprise is 0.005 (.001) in the unmatched sample and 0.006 (0.002) in the matched sample (untabulated), using the earliest forecast for each firm year. Using the latest management forecast of each firm year, the mean (median) absolute size of the forecast surprise is 0.005 (.001) in both the unmatched and matched sample (untabulated). Hence, the size

[36] Recall, from section 3.3.1, that the H1 sample is restricted to management forecasts issued on or after the prior years' earnings announcement, which limits *MF_HORIZON* to below 365 days.

of the earliest forecast surprise is rather similar to that of the latest forecast surprise.

To examine whether my choice of forecast horizon indeed affects results, I test H1 using the earliest management forecast for each firm-year, as opposed to the latest, in the constant derivative samples.[37] Before, I perform this analysis, I first compare the mean management forecast- and firm-level variables between control and treatment firms to determine which variables I need to include in the regression analyses. As discussed in section 3.2.2, I only include those variables that are statistically different between treatment and control groups to avoid including an excessive number of regressors in the model unnecessarily.

Table 4.11 reports the results of the t-test of mean differences. Panels A.1, A.2 and A.3 (B.1, B.2 and B.3) provide the descriptive statistics and t-test results for the combined, good news and bad news constant derivative unmatched (matched) sample/subsamples, respectively. Based on these results, I control for *OCFVOL*, *MF_LOSS, MVE* and *EA_CONCUR* in the unconditioned regression, using the constant derivative unmatched sample (see panel A.1). In the conditioned regression using the constant derivative unmatched sample, I retain *OCFVOL, MF_LOSS, MF_HORIZON, EA_CONCUR* and *MVE* as controls as these variables differ significantly between treatment and control groups within either the good news or bad news unmatched subsamples (see panels A.2 and A.3). Finally, using the constant derivative matched sample, I include a control for *MVE* only in both the unconditioned (see panel B.1) and conditioned regressions (see panels B.2 and B.3).

Table 4.12 reports the results of testing H1, using the earliest management forecast,

[37] Management forecast surprise is calculated as the forecast EPS minus the the mean analyst forecast EPS in the set of analyst forecasts issued 90 to 2 calendar days prior to the management forecast date, deflated by the pre-management forecast share price, as discussed in section 3.2.2. Thus, the earliest management forecasts reference a different set of analyst forecasts than the latest forecasts, if the firm issued more than one forecast in a given year.

instead of the latest, in a given firm-year, within the constant derivative samples (table 4.4). In the unconditioned regression, in panel A of table 4.12, the coefficient for *TREAT×POST× MF_SURP* is statistically insignificant, consistent with the primary results in panel A of table 4.6. In the conditioned regression, in panel B, I find a negative and significant coefficient on *TREAT×POST× MF_SURP_GNEWS* in the unmatched sample (column (1): β = -10.970; t-stat = -2.63), consistent with the primary results in panel B of table 4.6. However, in the matched sample, the coefficient is negative, but not statistically significant (column (2): β = -4.762; t-stat = -1.28), which differs from the statistically significant results in table 4.6. I find that the size of the negative coefficients for *TREAT×POST× MF_SURP_GNEWS*, are much smaller using the earliest forecasts than using the latest forecasts (table 4.6, panel B). However, I also find that the coefficients observed for *MF_SURP_BNEWS* and *POST× MF_SURP_GNEWS* are also smaller using the earliest forecasts than using the latest forecasts, in 3 out of 4 instances. Given that the size of the coefficients are smaller using earlier forecasts than using later forecasts, not only on *TREAT×POST× MF_SURP_GNEWS,* but also on other coefficients that capture forecast surprise, the results suggest an overall muted response to earlier forecasts that have more uncertainty. Next, I find a statistically insignificant coefficient on *TREAT×POST× MF_SURP_BNEWS,* consistent with the primary results in panel B of table 4.6. In sum, using the earliest forecast for each firm-year, I find weaker and inconsistent results supporting H1 for good news forecasts, relative to using the latest forecast.

4.2.3.5 Alternative Set of Control Variables

In the analysis of H1 using the constant derivative samples, in table 4.6, I retain the same set of controls that I used for the full samples, defined in panel B of table 3.1 and analyzed in table 4.3, because the constant derivative sample is a subset of the full sample. Recall that I excluded, from the regression model, control variables that were not statistically different between the treatment and control groups in panels C.1 to D.3 of table 4.2 for the respective samples. However, it is possible that the t-tests of mean differences in management forecast and firm characteristic variables in the constant derivative samples yield results that are different from those of the full samples. Hence, in this section, I re-perform the t-test of means using the constant derivative samples to identify an alternative set of control variables for the constant derivative sample regressions.[38]

Table 4.13 reports the results of the t-test of mean differences using the constant derivative samples. Panels A.1, A.2 and A.3 (B.1, B.2 and B.3) provide the descriptive statistics and t-test results for the combined, good news and bad news constant derivative unmatched (matched) sample/subsamples, respectively. Based on these tests, I include, in the respective regressions, only those variables that statistically differ between control and treatment groups. Hence, in the unconditioned regression using the constant derivative unmatched sample, I control for the following variables: *OCFVOL, MF_LOSS, MVE*, and *EA_CONCUR* (see panel A.1). In the constant derivative matched sample, I only control for *MVE* (see panel B.1). Next, in the conditioned regression using the constant derivative unmatched sample, I control for the following variables: *OCFVOL, MF_HORIZON*, and *MVE* (see panels A.2 and A.3). Finally, in the conditioned regression using the constant derivative matched sample, I control for *MF_HORIZON* and *MVE* (see panels B.2 and B.3). As discussed in section 3.2.2, I also include the interactions of these variables with the forecast surprise variables. I transform all continuous variables to indicator variables to facilitate the interpretation of these interaction variables.

[38] In table D.1 of appendix D, I include *all* control variables, regardless of whether or not they are statistically different between treatment and control groups, in the regression using the constant derivative unmatched sample. I find that the primary results are robust to this alternative specification of the model.

Table 4.14 provides the analysis of H1 using the constant derivative samples and the alternative set of control variables, identified using the t-test of means in table 4.13. In the unconditioned regression, in panel A, I find that the coefficients for *TREAT×POST× MF_SURP* are statistically insignificant in both the unmatched and matched samples, consistent with the primary results, reported in panel A of table 4.6. In the conditioned regression, in panel B, the coefficient for *TREAT×POST× MF_SURP_GNEWS* is negative and statistically significant (β = -13.770; t-stat = -1.52) in the constant derivative matched sample (column (2)), consistent with the primary results in table 4.6. However, the significance is smaller than that of the primary results. In the constant derivative unmatched sample (column (1)), *TREAT×POST× MF_SURP_GNEWS* is statistically insignificant (β = 0.797; t-stat = 0.16), in contrast to the primary results, which showed a negative and statistically significant coefficient. The coefficient for *TREAT×POST× MF_SURP_BNEWS* continue to be insignificant, consistent with the primary results. In sum, the primary results, reported in panel B of table 4.6, are not robust to the alternative set of control variables.

To explore the reason for the different results using the same sample, but different sets of control variables, in tables 4.6 and 4.14, I re-examine the conditioned regression, adding the control variables (and their interactions with the forecast surprise variables) in table 4.6, one at a time. These results are reported in panel B of table D.5 in Appendix D. I find that the coefficient for *TREAT×POST× MF_SURP_GNEWS* becomes negative, although statistically insignificant, (β = -8.26; t-stat = -1.16) only after including *MF_LOSS* and its interactions with the good and bad news forecast surprises. Hence, it appears that loss forecast observations are very influential. To further investigate if loss forecasts are responsible for the different results, I perform two additional analyses. First, I repeat the regression analyzed in panel B of table 4.14, using the alternative set of control variables, but also adding *MF_LOSS* and its interactions with *MF_SURP_GNEWS* and *MF_SURP_BNEWS*. If the null result in the constant derivative unmatched sample, in table 4.14, is indeed attributable to loss forecasts, I should observe a negative and statistically significant coefficient for *TREAT×POST× MF_SURP_GNEWS* once I control for *MF_LOSS*. Second, I repeat the regression, with the alternative set of control variables, but after excluding loss forecast observations. Specifically, this excludes 11 and 1 observations in the constant derivative unmatched and matched samples, respectively. Again, I expect to observe a negative and significant coefficient for *TREAT×POST× MF_SURP_GNEWS,* consistent with the primary findings (table 4.6, panel B), if the weak results in table 4.14 are attributable to influential loss forecasts.

Table 4.15 reports the results of these regressions. Column (1) reports the regression results including the alternative set of control variables, plus controls for *MF_LOSS* and its interactions with *MF_SURP_GNEWS* and *MF_SURP_BNEWS.* Once I control for *MF_LOSS*, I find that the coefficient for *TREAT×POST× MF_SURP_GNEWS* is indeed negative and significant at the 5% level (β = -14.492; t-stat = -2.11), similar to the primary results in panel B of table 4.6. Next, columns (2) and (3) report the regression results using the alternative set of control variables and excluding loss forecast observations. In the constant derivative unmatched sample (column (2)), the coefficient for *TREAT×POST× MF_SURP_GNEWS* is negative and statistically significant at the 1% level (β = -17.666; t-stat = -2.39). Hence, excluding the loss forecasts yield results that are consistent with the primary results, reported in table 4.6, but with a higher significance level. Results in the constant derivative matched sample (column (3)) are similar to both the results in table 4.14 and the primary results in table 4.6. These results, particularly in the constant derivative unmatched sample, indicate that loss forecast observations are, indeed, very influential. Their exclusion alters the coefficient for *TREAT*×*POST*× *MF_SURP_GNEWS* significantly.

Based on these results, I conclude that the primary results are not robust to the alternative set of control variables that exclude *MF_LOSS.* The primary results are, however, robust to the alternative set of controls, once I also control for the influence of forecast loss observations either by way of including *MF_LOSS* and its interactions with the forecast surprise variables as controls in the regression model or by excluding these observations. Accordingly, I caveat that excluding a control for *MF_LOSS* can alter the results observed in tables 4.6, 4.7 and 4.10. Furthermore, given the influential nature of loss forecast observations and the extremely small number of such observations in the constant derivative sample (i.e., 11 and 1 in the unmatched and matched samples, respectively), the primary findings may not generalize to loss forecasts.

4.2.3.6 Other Additional Analyses

In addition to the above, I carry out some six other additional analyses. The first four analyses, in tables D.1 to D.4, test the sensitivity of the primary findings, in table 4.6, to various specifications of the regression model. The fifth analysis, reported in table D.5, provides regression results including one control variable (and its interaction(s) with *MF_SURP* or *MF_SURP_GNEWS* and *MF_SURP_BNEWS*), at a time. This analysis allows me to better assess the reason for changes in the coefficients of interest. Finally, the last analysis, reported in table D.6, tests the robustness of the primary results to using a three-day cumulative abnormal returns (CAR) around the MF date, which includes the one day prior to the MF date, instead of a two-day CAR, which only includes the MF date and the day after.

First, table D.1 reports the results using the full set of control variables, regardless of whether they differ between treatment and control groups, using the constant derivative unmatched sample. As discussed in section 3.2.2, I exclude from the main regression model those control variables that are not statistically different between the treatment and control groups to limit the number of unnecessary independent variables. Results are similar to the primary results, reported in table 4.6. Hence, the inclusion of control variables considered and excluded in the main regressions do not alter results. I do not run this regression using the constant derivative matched sample because this can lead to overfitting. The constant derivative matched sample comprises only 219 observations and the unconditioned (conditioned) regression includes 28 (45) regressors.

Second, table D.2 reports the results including industry fixed effects and their interactions with *MF_SURP* (*MF_SURP_GNEWS* and *MF_SURP_BNEWS*). Given substantial variation in the proportion of derivative users across industries (see table 4.1), I examine whether including industry fixed effects alters results. I do not perform this analysis using the constant derivative matched sample as industry effects are already controlled in this sample by way of matching control and treatment firms within industry. Using the constant derivative unmatched sample, results are qualitatively similar to the primary findings, reported in table 4.6, with the following two exceptions. First, the coefficient for *TREAT* × *POST* × *MF_SURP*, in column (2) of panel A, is negative and statistically significant at the 10% level, showing support for H1, in the unconditioned regression. Second, the coefficient for *TREAT* × *POST* × *MF_SURP_BNEWS* is negative and statistically significant at the 10% level. Note that the primary results, in table 4.6, showed null results for both of these coefficients. Overall, these results show stronger support for H1 than the primary results. The primary finding that an increase in exposure to fair value accounting reduces the credibility of good news management forecasts is robust to including industry fixed effects.

Third, table D.3 reports the results using *continuous* control variables and their interactions with *MF_SURP* (*MF_SURP_GNEWS* and *MF_SURP_BNEWS*), instead of the indicator forms of the control variables. While interactions of continuous variables with *MF_SURP* (*MF_SURP_GNEWS* and *MF_SURP_BNEWS*) are difficult to interpret, it preserves more information about the controls than the indicator variables. These results are qualitatively similar to the main results in table 4.6, suggesting that using continuous control variables, instead of the transformed indicator variables, does not alter results.

Fourth, table D.4 reports the results excluding the stand-alone control variables, and only including the *MF_SURP*- (*MF_SURP_GNEWS*- and *MF_SURP_BNEWS*-) interacted controls to be more consistent with the regression model in Rogers and Stocken (2005). All results are qualitatively consistent with the primary results reported in table 4.6.

Next, table D.5 reports the results including one control variable (and its interaction(s) with *MF_SURP* or *MF_SURP_GNEWS* and *MF_SURP_BNEWS*) at a time. This analysis is not aimed to test the robustness of the primary results, but rather as a supplemental analysis, that can be used to inform the potential reason for changes in the coefficients of interest to the inclusion or exclusion of certain variables. Various parts of section 3.2.2, 4.2.2 and 4.2.3 refer to this table.

Finally, I examine the robustness of the primary results, in table 4.6, to using a three-day CAR instead of a two-day CAR. This analysis allows me to assess whether including the day before the MF date affects results. If there is a leakage of news on the day prior to the MF date, results may be stronger using the three-day CAR, relative to using the two-day CAR. In contrast, if including the day prior to the MF date adds noise, I expect results to be weaker using the three-day CAR than using the two-day CAR. These results, reported in table D.6, are similar to the primary findings in table 4.6, with one exception. In panel A, I find that the coefficient for *TREAT* × *POST* × *MF_SURP* in the constant derivative matched sample (column (2)) is negative and statistically significant at the 5% significance level. Hence, I find results in support for H1, even in the unconditioned regression, when I use the three-day CAR instead of the two-day CAR. This result suggests that there may be some news leakage in the day prior to the MF date.

Overall, I find that primary results, in table 4.6, are robust to the following additional alternate regression model specifications, reported in appendix D: (1) including all control variables for the unmatched sample; (2) including industry fixed effects and their interactions with *MF_SURP* (*MF_SURP_GNEWS* and *MF_SURP_BNEWS*); (3) using continuous control variables instead of indicator variables; and (4) excluding stand-alone control variables and only including interactions of control variables with *MF_SURP* (*MF_SURP_GNEWS* and *MF_SURP_BNEWS*). The primary results are also robust to using a three-day CAR instead of a two-day CAR. When results differ from the primary results, they suggest stronger support for H1.

4.3 Fair Value Accounting and Timeliness of Price Discovery (H2)

This section presents the results of the empirical analyses examining the impact of fair value accounting on the timeliness of price discovery. Section 4.3.1 provides the industry distribution and compares potential confounding variables between the control and treatment groups in the unmatched and matched H2 samples. Section 4.3.2 presents the results of the tests of H2, which show that fair value accounting does not affect the timeliness of price discovery during periods of positive intraperiod returns and increases the timeliness of price discovery during periods of negative intraperiod returns. Section 4.3.3 presents the results of additional analyses that test the robustness of the primary results, reported in section 4.3.2, to alternative matched samples. In addition, I perform addition analyses to explore potential explanations for the primary results.

4.3.1 Descriptive Statistics

4.3.1.1 Industry Distribution

I examine the industry distribution and the proportion of treatment firms within each industry in the unmatched H2 sample, identified in panel B of table 3.4, to assess derivative usage. Table 4.16 reports the frequency distribution of unique firm observations in the unmatched and matched H2 samples, by industry and treatment. In the unmatched sample, the following industries have the largest representation: manufacturing, business equipment, healthcare and other. However, after matching, the healthcare industry is no longer one of the top four most well represented industries in the sample due to the large imbalance of derivative users and non-users in this industry. Recall that, because I match within industry, the extent of imbalance in derivative users and non-users, within industry, affects the composition of the matched sample. In contrast, the consumer durables, chemicals and allied products, and telecommunications industries have the smallest representation in both the unmatched and matched samples.

Next, I examine the proportion of treatment firms in each industry to assess derivative usage. In the unmatched sample, derivative users comprise the largest proportions of the consumer non-durables, energy and extraction, and utilities industries. These industries have more derivative users than non-users in the unmatched sample. In contrast, derivative users comprise less than 25% of the following industries: business equipment and healthcare. Overall, a little over one-third of the unmatched H2 sample comprises derivative users.

4.3.1.2 Comparison of Mean Potential Confounding Variables between Control and Treatment Groups

I examine differences between the treatment and control groups for variables representing potential confounds – namely operational uncertainty and the richness of the information environment. As discussed in section 3.2.3, I use *OCFVOL* to proxy for operational uncertainty and *MVE* as an indicator for the richness of the information environment. Table 4.17 compares these variable means between control and treatment groups in the pre-period. I also examine analyst following, *ANALYSTS_N,* calculated as the natural log of one plus the number analysts following the firm for the 12-month period, as an alternative proxy for the richness of the information environment. Panels A and B report the tests of mean differences in confounding variables between the control and treatment groups in the unmatched and matched H2 samples, respectively. Panels C and D report the test results for the positive and negative intraperiod return matched subsamples, respectively. The positive (negative) intraperiod return subsample includes observations whose 12-month buy-and-hold abnormal return is positive (negative) in both the pre- and post-periods.

In panel A, the mean *OCFVOL* in the treatment group is less than half of that in the control group, prior to matching. Hence, derivative users have substantially lower operational uncertainty than non-users, suggesting that derivative users, in the sample, use derivatives to effectively hedge their risk exposures. In section 3.2.2, I argued that derivative users may have greater or lower operational uncertainty depending on whether or not their derivatives are effective hedges. In panel A, I also find that treatment firms have a significantly larger mean

MVE and *ANALYSTS_N* than controls firms, indicating that treatment firms have a richer information environment than control firms. This is consistent with prior literature (e.g., Guay 1999; Zhang 2009; Donohoe 2015; Chang et al. 2016) that finds that derivative users tend to be larger and have greater analyst following than non-users. In panel B, after matching on *OCFVOL* and *MVE* (in addition to industry and sign of intraperiod return), I find no significant differences in the means, suggesting a successful match. In addition, the lack of a statistically significant difference in *ANALYSTS_N* suggests that matching on *MVE* also controls for that indicator of the information environment. In fact, *MVE* and *ANALYSTS_N* are highly positively correlated (untabulated - pearson: $\rho = 0.810$; spearman: $\rho = 0.816$). In the positive return subsample (panel C), I find no significant differences in the variable means between the control and treatment groups. In the negative return subsample (panel D), treatment firms have a marginally larger *ANALYSTS_N* than control firms.[39]

4.3.2 The Effect of Fair Value Accounting on the Timeliness of Price Discovery

To test the effect of exposure to fair value accounting on the timeliness of price discovery, I compare the change in *IPT* from the pre- to the post-SFAS 133 period between derivative users (treatment) and non-users (control). That is, I estimate the *DiD_IPT* as:

$$(IPT_{treat,post} - IPT_{treat,pre}) - (IPT_{control,post} - IPT_{control,pre})$$

I then test the significance of the *DiD_IPT* using a null distribution of *DiD_IPT* where the ordering of the monthly returns in each portfolio is randomized, as discussed in section 3.2.3.2. Appendix E reports the null distributions for these tests. H2 predicts that greater exposure to fair value accounting reduces the timeliness of price discovery. Consistent with H2, I expect to

[39] In section 4.3.3.1, I test whether matching on *ANALYSTS_N* instead of *MVE,* in addition to *OCFVOL*, industry and sign of intraperiod return, affects inferences, and find no qualitative difference.

observe a negative *DiD_IPT*, suggesting that the change in information timeliness is more negative for the treatment portfolio than for the control portfolio around the implementation of SFAS 133.

Table 4.18 presents the results of the test of H2. I test H2 using the separate positive intraperiod return and negative intraperiod return subsamples, in addition to the combined sample of positive and negative return observations. I separately examine the positive and negative intraperiod return subsamples because, the confirmability of financial reports may be more important for firm-years with a net positive return than for those with a net negative return. As discussed in section 3.2.2, management-issued bad news is inherently more credible than management-issued good news.

Panel A of table 4.18 presents the results using the unmatched H2 sample. In the combined sample, *ΔIPT* is positive and statistically significant at the 5% significance level in both the treatment and control portfolios, indicating that timeliness improved for both groups. The *DiD_IPT* is not statistically significant because increases in *IPT* are similar in the two portfolios.[40] The increase in *IPT* is consistent with the overall increase in voluntary disclosures with the enactment of Reg FD in 2000 documented in prior literature (e.g., Heflin et al. 2003; Anilowski et al. 2007; Choi et al. 2010). In the positive intraperiod return subsample, *ΔIPT* is positive in both the treatment and control portfolios, but statistically significant in the control portfolio only. This results in a negative and statistically significant *DiD_IPT* at the 10% significance level, consistent with H2. In the negative intraperiod return subsample, *ΔIPT* is again positive in both portfolios, but statistically significant in the treatment portfolio only,

[40] The corresponding null distributions of *DiD_IPT* and *ΔIPT* are presented in appendix E.1, for reference.

resulting in a positive and significant *DiD_IPT* at the 1% significance level, contrary to H2. These preliminary univariate results in the unmatched sample provide support for H2 in the positive return subsample. However, in the negative return subsample, the results contradict H2.

Panel B of table 4.18 presents the results using the matched H2 sample, which controls for operational uncertainty and the richness of the information environment. Using the combined sample, both the control and treatment portfolios experience an increase in *IPT*. While the *ΔIPT* is larger in the control portfolio than in the treatment portfolio, the *ΔIPT* is statistically significant in the treatment portfolio only. The resulting *DiD_IPT* is negative, but not statistically significant. In the positive intraperiod return subsample, both the control and treatment portfolios increase in *IPT,* but statistically insignificantly so. This *DiD_IPT* is negative but statistically insignificant. In the negative intraperiod return subsample, *ΔIPT* is positive and statistically significant in both control and treatment portfolios, but the *DiD_IPT* is insignificant due to similar *ΔIPT* in the two portfolios. Overall, I fail to find support for H2 using the matched sample.

To assess whether return reversals influence *IPT*, I plot the IPT curves for the four portfolios: treatment-pre, treatment-post, control-pre and control-post. As I discuss in appendix B, non-trivial return reversals can inflate *IPT* values, rendering the *IPT* metric problematic for interpreting the timeliness of price discovery. Figures 4.1 and 4.2 presents the *IPT* curves for the unmatched and matched H2 samples, respectively. Each figure contains three plots: for the combined sample (panel A) and each of the positive and negative (panels B and C, respectively) intraperiod return subsamples.

In appendix B, I find that portfolios with even 50 observations average out idiosyncratic return reversals to a reasonable range (*RET_REV* below two). However, portfolios with 50 observations contain return reversals that are notably larger than those in portfolios with 100 or 200 observations. Thus, it is better to use larger portfolio sizes to minimize the impact of reversals. All portfolios represented in figures 4.1 and 4.2 have greater than 50 observations and, thus, are deemed reasonable for interpreting *IPT*. Nevertheless, differences in the extent of return reversals between two portfolios with different sample sizes can influence results. Therefore, one should use caution when interpreting *IPT* for smaller portfolios (i.e., fewer than 500 observations).[41] I focus the following discussion on non-trivial return reversals that comprise greater than 5% of the 12-month buy-and-hold return.

Figure 4.1 plots IPT curves for the unmatched sample/subsamples. Panel A of figure 4.1 presents the *IPT* curves for the combined unmatched sample, and displays no return reversals throughout the period.[42] Hence, *IPT*s in the combined sample are not influenced by return reversals and can be interpreted as the timeliness of price discovery. Panel B presents the *IPT* curves for the positive intraperiod return subsample. In this subsample, the post-period treatment portfolio has return reversals between months -5 and -3, which amount to almost 15% of the 12-month buy-and-hold return. This tends to bias against finding results supporting H2 by inflating the post-period treatment portfolio's *IPT* value and, thus, making the *DiD_IPT* more positive than if the curve were monotonically increasing. Recall that H2 predicts a negative *DID_IPT*. Finally, panel C presents the *IPT* curves for the negative intraperiod return subsample. In this subsample, the pre-period treatment portfolio has a return reversal between months 2 and 3 that comprises nearly 10% of the 12-month buy-and-hold return. This can bias for finding results

[41] In appendix B, I find that once the portfolio size become 500 or greater, mean *RET_REV* equals one, indicating zero return reversals. Thus, *IPT* can be interpreted reliably in these portfolios.

[42] To be clear, the cumulative % 12-month buy-and-hold return is 50.0% at month -4 and 50.2% at month -3 (untabulated). So, there is no return reversal between these months.

supporting H2 by inflating the pre-period treatment portfolio's *IPT* value, resulting in a less positive *DiD_IPT*.

Figure 4.2 presents analogous *IPT* curves for the matched H2 sample/subsamples. In panel A, the combined sample shows no return reversals larger than 5% of the 12-month buy-and-hold return. Hence, *IPT* can be reasonably used to assess the timeliness of price discovery. In the positive return subsample (panel B), the post-period treatment portfolio again has a return reversal between months -5 and -3, similar to that observed in the unmatched H2 sample in panel B of figure 4.1. However, this reversal is larger than that observed in the unmatched H2 sample; it comprises almost 22% of the 12-month buy-and-hold return. The greater salience of the reversal in the matched H2 sample, relative to the unmatched H2 sample, may be related to a smaller sample size. The control and treatment portfolios in the unmatched positive return subsample comprise 274 and 123 observations, while those in the matched positive return subsample comprise 66 observations each. As discussed in section 3.2.3 and in appendix B, a larger sample better averages away the idiosyncratic returns at the firm level, alleviating return reversals. In particular, as documented in appendix B, while portfolios with 50 observations reduce return reversals substantially, the maximum return reversal (panel B of table B.2: 1.937) approaches two, a value that I deem to be problematic for using *IPT* to assess timeliness of price discovery. The post-period control portfolio also exhibits a return reversal between months -4 and -3 that comprises almost 7% of the 12-month buy-and-hold return. This partially mitigates the influence of the return reversal in the post-period treatment portfolio on the *DiD_IPT*. Nevertheless, the return reversal in the post-period treatment portfolio is larger and, thus, still tends to bias against finding results for H2 by making the *DiD_IPT* more positive.

In the negative return subsample (figure 4.2, panel C), both the pre-period treatment and control portfolios exhibit return reversals between months 2 and 3. However, the return reversals are very similar in magnitude and, thus, the net effect on the DiD is minimal. In fact, the IPT curves of the treatment and control portfolios are very similar in both the pre- and the post-periods, resulting in a very small and insignificant *DID_IPT*, as observed in panel B of table 4.18.

Overall, in the combined samples, no return reversals exceed 5%. Hence, the *DID_IPT* is reliable for interpreting the effect of exposure to fair value accounting on the timeliness of price discovery. In the positive return subsamples, the post-period treatment portfolio exhibits non-trivial return reversals that comprise nearly 15% and 22% of the 12-month buy-and-hold return in the unmatched and matched samples, respectively. This can potentially bias against finding results supporting H2, by influencing the *DID_IPT* to be more positive. The larger return reversal in the positive return subsamples than in the combined samples or the negative return subsamples may be related to smaller sample sizes that fail to sufficiently average away the random arrival of firm-level news. Thus, the insignificant results in the matched positive return subsample, in panel B of table 4.18, may be associated with a small sample size. Nevertheless, the return reversal in this portfolio is 1.433 (untabulated), which is well below the clearly problematic *RET_REV* of two, identified in appendix B.4. Thus, the *IPT* values in the positive return subsample can still be useful, if interpreted with caution. In the negative return subsamples, the impact of non-trivial return reversals on *IPT* are alleviated using the DiD research design because the reversals are similar between the treatment and control portfolios in both the pre- and post-periods. Thus, the *IPT* results in the negative return subsample are reliable for interpreting the timeliness of price discovery.

In summary, I fail to find results supporting H2 in the matched sample, suggesting that fair value accounting does not affect the timeliness of price discovery, once I control for operational uncertainty and the richness of the information environment. A potential reason for the lack of evidence supporting H2 is that some of the treatment (control) firms stop (begin) using derivatives in the post-period. As seen in section 4.2.2.1, around half of the control firms and a non-trivial portion of the treatment firms in the H1 sample change their decision to use/not use derivatives in the post-period. To alleviate concerns that such changes bias against finding results supporting H2, in the following section, I test H2 after excluding firms that change their decision to use or not use derivatives.

4.3.2.1 Exclude Firms that Change Decision to Use/Not Use Derivatives

In this section, I test H2 using constant derivative samples, where I keep only firms that continue to use (not use) derivatives, similar to those used to test H1 in section 4.2.2.2. This ensures that the observed results, in section 4.3.2, are not confounded by changes in firms' decisions to use (not use) derivatives and preserves a constant sample of treatment and control groups, as discussed in section 3.2.

Panel A of table 4.19 reports the sample selection process for the constant derivative unmatched H2 sample. Of the 885 control firms in the unmatched H2 sample, 151 begin using derivatives in the post-period. As discussed in section 4.2.2.1, Abdel-Khalik and Chen (2015) argue that firms increased their use of non-trading derivatives post-SFAS 133 to reduce earnings volatility. Similar to H1, this raises self-selection concerns. However, this likely introduces a conservative bias because a decrease in earnings volatility likely decreases informational asymmetry, which, in combination with more credible disclosures, discussed in section 4.2.2.1, facilitates more timely price discovery. Thus, this biases against finding results in support of H2. Next, 27 out the 492 treatment firms stop using derivatives in the post-period. Similar to H1, this proportion is much smaller than the proportion of control firms that begin using derivatives in the post-period.

The proportion of control firms that begin using derivatives (151 out of 885 firms) in the H2 sample is much smaller than that in the H1 sample where 44 out of 97 control firms stopped using derivatives (see table 4.4, panel A). This is likely associated with the fact that the H1 sample only includes firms that issue management forecasts, which tend to be larger. Larger firms are more likely to use derivatives in the post-period, as discussed in section 4.2.1.1. The average pre-period market value of equity in the H1 unmatched sample is $11.067 billion, whereas it is only $2.164 billion in the H2 unmatched sample (untabulated).

Panel B of table 4.19 reports the constant derivative matched H2 sample. To create a constant derivative matched sample, I perform a new match within the constant derivative unmatched H2 sample, shown in panel A of table 4.19, with 734 and 465 control and treatment firms, respectively. As discussed in section 3.3.2, I match on *OCFVOL* (26 cutpoints), *MVE* (10 cutpoints), industry and sign of intraperiod return, using CEM. This matching process yields 224 matched pairs of control and treatment firms.

Panel A of table 4.20 presents the results of testing H2 using the constant derivative unmatched sample. Results remain qualitatively unchanged from the results in panel A of table 4.18 using the unmatched sample. In the positive intraperiod return subsample, the *DiD_IPT* is negative and statistically significant at the 10% significance level and, in the negative intraperiod return subsample, the *DiD_IPT* is positive and statistically significant at the 1% significance level. The combined sample yields null results due to the opposite effects found in the positive and negative return subsamples.

Next, I examine the results using the constant derivative matched sample in panel B of table 4.20. I continue to find an insignificant *DiD_IPT* in the combined sample and the positive return subsample, similar to the results in panel B of table 4.18. However, the *DiD_IPT* is positive and statistically significant at the 1% significance level in the negative return subsample, which differs from the insignificant *DiD_IPT* in table 4.18 (panel B) and contradicts H2.

In figure 4.3, I plot the *IPT* curves for the constant derivative matched sample to look for non-trivial return reversals that may render the *IPT* metric problematic for interpreting the timeliness of price discovery, as discussed in appendix B. For brevity, I only highlight notable differences in return reversals between the constant derivative matched sample *IPT* curves and the corresponding *IPT* curves for the full matched sample in figure 4.2, discussed above.

Panels A, B and C of figure 4.3 presents the *IPT* curves for the combined, the positive intraperiod return and the negative intraperiod return constant derivative matched sample/subsamples, respectively. There are no notable differences in return reversals between the constant derivative matched sample and the full matched sample (figure 4.2) using the combined sample or the positive intraperiod return subsample. In the positive return subsample (figure 4.3, panel B), I continue to find a non-trivial return reversal in the post-period treatment portfolio.

In the negative intraperiod return subsample (figure 4.3, panel C), the post-period control portfolio has a return reversal between months -6 and -5 that comprises almost 6.8% of the 12-month buy-and-hold return, which was not observed in the full sample. This can inflate the post-period *IPT* value for the control group. However, the post-period treatment portfolio also has a return reversal between months -6 and -5, which comprises 3.4% of the 12-month buy-and-hold return, offsetting some of the return reversal observed in the post-period control portfolio. In particular, an inflation of the *IPT* value for the post-period control group due to return reversal biases for finding a negative *DiD_IPT*. However, I find a positive, not a negative, *DiD_IPT* in the negative return subsample; hence, the observed results are not driven by the return reversal in the post-period control portfolio.

Importantly, restricting the sample to firms that continue to hold/not hold derivatives widens the gap between the post-period treatment and control portfolio *IPT* curves within the negative return subsample. In panel C of figure 4.2, the two curves are so close that they nearly overlap each other, resulting in a small and insignificant *DiD_IPT*. In the constant derivative sample, the higher post-period treatment IPT curve and the lower post-period control IPT curve, relative to the full sample, result in a positive and statistically significant *DiD_IPT*. Thus, it appears that including firms that changed their decision to use/not use derivatives biased the *DiD_IPT* to be more negative in this subsample.

In summary, I find results supporting H2 in the unmatched H2 sample. However, the results continue to fail to provide evidence supporting H2 in the matched sample, even after restricting the sample to constant derivative users and non-users, suggesting that an increase in fair value accounting exposure does not hinder timely price discovery, once I control for potential confounds. In fact, in the negative return subsample, I find results opposite to H2, suggesting that fair value accounting increases the timeliness of price discovery. This contradictory result suggests that fair value accounting affects the confirmability of financial reports and, thus, intraperiod timeliness in a manner that differs from my expectations, when the net intraperiod news is negative.

Another potential interpretation of the null results in the positive return subsample and the positive *DiD_IPT* in the negative return subsample is that increased disclosure requirements under SFAS 133 enhanced the transparency of derivative use, increasing, rather than decreasing, the confirmability of financial reports.[43] It is possible that SFAS 133 enhances the external information environment (e.g., analysts) of derivative users, enhancing the timeliness of price discovery, relative to non-users. Finally, another potential explanation for these results is that the effects of Reg FD are not constant for derivative users and non-users, as I assumed. If Reg FD increases voluntary disclosures more for derivative non-users than for users, this can bias against finding results for H2 or produce results in the opposite direction from H2.

Furthermore, in the positive return subsample, an alternative explanation for the null result is that the sample size is too small to sufficiently average away the firm-level idiosyncratic returns. Recall from section 3.2.3.1 that averaging away random news arrival at the firm level is crucial for interpreting *IPT* as the timeliness of price discovery. Note that the constant derivative matched sample portfolios sizes (N = 50) are much smaller than those of the unmatched sample (Control: N = 225; Treatment: N = 120) and, in the unmatched sample, I find results supporting H2. As documented in appendix B (panel B of table B.2), the mean (median) return reversal (*RET_REV_Port*) in portfolios with 50 observations is 1.066 (1.027), while that in portfolios with 100 observations is 1.016 (1.000). Hence, moving from a portfolio size of 50 observations

[43] I find support for the increase in disclosures around derivatives. In treatment firms that continue to use derivatives in the post-period, within the unmatched H2 sample, the mean (median) number of derivative words increased from 21 (16) words in the pre-period to 36 (28) words in the post-period (untabulated).

to that of 100 observations can affect the extent of return reversals in intraperiod portfolio returns.[44] I discuss potential explanations for the observed results in greater detail in section 4.4.

4.3.3 Additional Analyses

In this section, I perform additional analyses to test the sensitivity of the primary findings, reported in panel B of table 4.20, to alternative matched samples. Recall that I find null results in the combined sample and the positive intraperiod return subsample and opposite results to H2 in the negative intraperiod return subsample. I explore potential explanations for the primary findings. To do so, I first examine the impact of SFAS 133 on the frequency of management forecasts because, in section 2.4.3, I conjecture that low financial report confirmability can impede timely price discovery by reducing management's propensity to issue voluntary disclosures. Next, I examine changes in analyst following around SFAS 133 to assess whether changes in non-management issued disclosures explain the observed results.

4.3.3.1 Alternative Matched Samples

In this section, I test the robustness of the primary results using the constant derivative matched sample, presented in panel B of table 4.20, to three alternative matched samples. I focus on the results using the constant derivative sample because a non-trivial proportion of firms in the full sample, identified in panel B of table 3.4, change their decision to use/not use derivatives in the post-period (see table 4.19, panel B). In the first two alternative samples, I separately control for each of *OCFVOL* and *MVE* to match on fewer dimensions, which provides a larger number of matches. While matching on multiple criteria likely produces a better control group that is balanced along a number of covariates, it results in a smaller sample size. The constant

[44] Recall, from appendix B, that *RET_REV_Port* equals one (minimum value) when there are no return reversals.

derivative *OCFVOL*-matched sample is identified using CEM on *OCFVOL* (44 cutpoints), industry and sign of intraperiod return, while the constant derivative *MVE*-matched sample is identified using CEM on *MVE* (10 cutpoints), industry and sign of intraperiod return. The constant derivative matched sample comprises 224 pairs of treatment and control firms (see table 4.19, panel B), while the constant derivative *OCFVOL*- and *MVE*-matched samples comprise 349 and 245 pairs, respectively. For the third alternative sample, I match on analyst following, *ANALYSTS_N,* instead of *MVE,* as an alternative proxy for the richness of the information environment. This sample is identified using CEM on *OCFVOL* (33 cutpoint), *ANALYSTS_N* (5 cutpoints), industry and sign of intraperiod return, and results in 257 pairs of treatment and control firms.

Panels A, B and C of table 4.21 present the results using the constant derivative *OCFVOL*-, *MVE*- and *OCFVOL*-*ANALYSTS_N*-matched samples, respectively. The results of the *OCFVOL*-matched sample, in panel A, are qualitatively similar to the primary results in panel B of table 4.20. Hence, dropping the control for *MVE* and consequently increasing the number of matches does not significantly alter results. I continue to find results similar to the primary results in the *MVE*-matched sample (panel B), except in the negative intraperiod return subsample. In this subsample, the *DiD_IPT* remains positive, but is not statistically significant as it is in the primary results. Hence, it is important to control for operational uncertainty. Finally, results are robust to using *ANALYSTS_N*, instead of *MVE*, to control for the richness of the information environment (panel C).

Larger portfolio sizes generally do not appear to alter results observed in the constant derivative matched sample, reported in panel B of table 4.20. In particular, in the positive return subsample, larger portfolio sizes do not produce results closer to those in the constant derivative unmatched sample, reported in panel A of table 4.20. However, these portfolio sizes (N = 88, 57 and 66 in panels A, B and C, respectively, of table 4.21) are still substantially smaller than those of the constant derivative unmatched sample (Control N = 225 and Treatment N = 120 in panel A of table 4.20). Hence, I cannot fully dispel the alternative explanation that the null results in the positive return subsample may be attributable to a small portfolio size. As discussed in section 4.3.2 and in appendix B, while portfolios with 50 observations noticeably reduce the impact of returns reversals to reasonable levels, smaller portfolios still contain larger return reversals than larger ones. This effect nearly disappears once the portfolio size reaches 500 observations (see table B.2).

4.3.3.2 Frequency of Management Forecasts

As discussed in section 2.4.3, managers may be less likely to issue voluntary disclosures when the financial report confirmability is dampened, and the credibility of voluntary disclosures are reduced. So, one of the ways in which, I posit, the confirmatory role of financial reports can impede timely price discovery is by reducing management's likelihood of issuing voluntary disclosures. In this section, I compare the change in the frequency of management forecasts from the pre- to the post-period between treatment and control groups. The variable for management forecast frequency, *MF_N*, equals the natural log of one plus the number of management forecasts issued in the same 12-month period as the *IPT* period. If the increase in exposure to fair value accounting associated with SFAS 133 for derivative users reduces management's likelihood of issuing voluntary disclosure, I expect that treatment firms will experience a more negative change in management forecast frequency from the pre- to the post-SFAS 133 period than control firms. That is, I expect the *DID_MF_N* ($\{MF\_N_{treat,post} – MF\_N_{treat,pre}\}$ - $\{MF\_N_{control,post} – MF\_N_{control,pre}\}$) to be negative. I examine this informal conjecture using the constant derivative matched H2 sample derived in panel B of table 4.19, which controls for operational uncertainty (*OCFVOL*) and the richness of the information environment (*MVE*) and excludes firms that change their decision to use/not use derivatives in the post-period.

Table 4.22 reports the *DID_MF_N* results. The *ΔMF_N* is positive and statistically significant in both control and treatment groups across all sample/subsamples. This is consistent with an increase in voluntary disclosures after Reg FD, as evidenced in Heflin et al. (2003) and discussed in section 3.2. In the combined sample and the negative intraperiod return subsample, the increase in management forecast frequency is slightly larger in the treatment group than in the control group, resulting in a positive, yet statistically insignificant, *DiD_MF_N*. In the positive intraperiod return subsample, the increase in management frequency is slightly larger in the control group than in the treatment group, resulting in a negative, yet statistically insignificant, *DiD_MF_N*.

In sum, I do not find support for my conjecture that an increase in exposure to fair value accounting reduces voluntary disclosure frequency. This suggests that exposure to fair value accounting does not affect the frequency of management forecasts. However, it is also possible that Reg FD increased management forecast frequency more for derivative users than for non-users, reversing any management forecast decreasing effects SFAS 133 may have had on derivative users.

4.3.3.3 Analyst Following

The theoretical causal construct in this thesis is the confirmatory role of financial reports and its effect on management-issued voluntary disclosures. However, the timeliness of price discovery includes the influence of other channels of information, such as financial analysts and the financial press. In this section, I focus on analysts as one of these alternative channels of information and explore whether analyst following can explain some of the observed results in section 4.3.2. Specifically, I compare the change in analyst following from the pre- to the post-SFAS 133 period between derivative users and non-users using a DiD t-test. Since analysts are major intermediaries of information, changes in analyst following can affect the timeliness of price discovery.[45] For example, Brown and Rozeff (1978), Fried and Givoly (1982) and O'Brien (1988) find evidence suggesting that analysts incorporate timely information into their forecasts. This analysis is exploratory in nature as I have no ex ante directional predictions about the DiD. However, if the change in analyst following is more positive for derivative users, the null results observed in the positive return subsample and the positive *DiD_IPT* observed in the negative return subsample, in panel B of table 4.20, may be driven by a change in the external information environment, rather than due to a change in the confirmability of financial reports.

Table 4.23 presents the results of the DiD t-test of the impact of SFAS 133 on analyst following using the constant derivative matched H2 sample (table 4.19, panel B). In the combined sample and the negative intraperiod return subsample, I find that both control and treatment groups have a negative *ΔANALYSTS_N*, suggesting a decrease in analyst following. Mohanram and Sunder (2006) find that analyst following decreased from the pre- (November 1999 to October 2000) to the post-Reg FD period (November 2000 to October 2001), which partially overlaps with my periods. Bradshaw, Ertimur and O'Brien (2017) highlight that, in addition to the enactment of Reg FD, investigations over analysts' conflicts of interest that led to

[45] See pages 416-418 of Healy and Palepu (2001) for a discussion of empirical research that examines the role of financial analysts as information intermediaries in capital markets. This review suggests that financial analysts enhance capital market efficiency.

the Global Analyst Research Settlement in 2003 also occurred during this period. Such changes may have contributed to a wide-spread drop in analyst following.

In contrast, I find no change in analyst following in either the control or treatment groups in the positive intraperiod return subsample. It is possible that a change in analyst following is only observed in negative intraperiod return periods, which reflect net bad news, because the role of analysts is of greater importance during these periods than in positive return periods, which reflect good news. Hong, Lim and Stein (2000) argue that the role of analysts is particularly important when firms have bad news because managers are more likely to withhold or be less forthcoming with bad news. In contrast, managers are more likely to openly communicate good news. Thus, if Reg FD made it more difficult for analysts to acquire firm-specific information, this effect is likely more pronounced for bad news periods. Furthermore, McNichols and O'Brien (1997) find evidence suggesting that analysts are more reluctant to issue bad news forecasts. Hence, in the face of greater information acquisition costs, analysts are more likely to drop coverage of firms with a poorer outlook (i.e., negative intraperiod returns).

However, in all sample/subsamples, the *DiD_ANALYSTS_N* is statistically insignificant: I find no significant difference in the change in analyst following around the SFAS 133 pronouncement between treatment and control groups. Hence, changes in analyst following do not explain the positive and significant *DiD_IPT* for the negative intraperiod return subsample, observed in panel B of table 4.20.

4.4 Conclusion

The empirical analysis in section 4.2 provides evidence supporting H1 for good news management forecasts. Specifically, I find that derivative users experience a decrease in credibility after an increase in their exposure to fair value accounting under SFAS 133, but only when the management forecasts convey good news. Fair value exposure does not appear to affect the credibility of forecasts when they convey bad news. Bad news disclosures may be less sensitive to confirmability of financial reports, relative to good news, because bad news from management is perceived to be inherently more credible. Given mixed evidence on the credibility of good news forecasts using the full sample, I caveat that these results may be limited to firms that do not change their decision to use/not use derivatives.

The findings for H1 are generally robust to including firm fixed effects and using an alternative matched sample. The results are strengthened once I exclude bundled management forecasts, suggesting that concurrent earnings announcements add noise to the test of H1. However, using the earliest management forecasts in a given year instead of latest forecasts yields weaker results, consistent with the expectation that the impact of a *reduction* in financial report confirmability will have greater test power using management forecasts that are otherwise more credible (i.e., shorter horizon forecasts). Interestingly, I find that the primary findings that fair value accounting reduces the credibility of good news management forecasts disappears when I include an alternative set of control variables. Additional analyses indicate that this loss in significance is driven by influential loss forecasts, which comprise less than 2% of the constant derivative sample. Once, I control for these observations or exclude them from the analysis, results are similar or stronger than the primary results. Finally, the results are robust to various alternate specifications of the regression model and to using three-day CAR, instead of two-day CAR. Overall, the results provide support that fair value accounting for derivative users lowers the credibility of good news management forecasts, highlighting a potential unintended consequence of fair value accounting.

Unlike the findings for H1, I fail to find evidence supporting H2 in section 4.3. Specifically, in the positive intraperiod return subsample, I find no statistical difference between derivative users and non-users in the change in *IPT* from the pre- to the post-SFAS 133 period, after controlling for potential confounds. In contrast, in the negative intraperiod return subsample, I find that derivative users experience a more *positive*, as opposed to the predicted negative, change in *IPT* around SFAS 133 than derivative non-users. These results suggest that fair value accounting may not affect, or even increase, the timeliness of price discovery. As discussed in section 2.4.3, fair values can enhance the timeliness of price discovery to the extent that they are credible, by including more current information in interim financial reports, relative to historical costs. Below, I discuss potential alternative explanations for the lack of evidence supporting H2.

First, it is possible that features of SFAS 133, other than fair value accounting, affect the confirmability of financial reports in the opposite direction from that predicted. Specifically, in addition to increasing fair value accounting exposure for derivative users, SFAS 133 also enhanced the transparency of derivatives and their use by standardizing the accounting for derivatives and the hedge accounting criteria, and increasing the required disclosures for derivatives. I posit that an increase in the transparency of derivative use can enhance the confirmability of financial reports if it better equips users to assess the accuracy or truthfulness of managers' non-verified earlier voluntary disclosures. This, in turn, can enhance the timeliness of price discovery, as discussed in section 2.4.3. Thus, the increase in transparency of derivative use under SFAS 133 provides a force opposite to my prediction in H2, and a potential explanation for the negative return subsample.

Second, the results on the impact of fair value accounting on the timeliness of price discovery may be confounded by Reg FD. I hypothesized that fair value accounting reduces the timeliness of price discovery by dampening the confirmability of financial reports. In turn, this reduces the credibility of voluntary disclosures and, thus, demotivates managers from issuing voluntary disclosures, contributing to less timely price discovery. I find evidence suggesting that fair value accounting reduces the credibility of voluntary disclosures (H1), as discussed above. However, in section 4.3.3.2, I fail to find evidence suggesting that fair value accounting reduces the frequency of voluntary disclosures using management forecasts. The average frequency of management forecasts increases in both control and treatment groups, likely due to the implementation of Reg FD in 2000, as evidenced in Heflin et al. (2003). While I aim to control for temporal effects, such as Reg FD, using a DiD research design, it's possible that Reg FD affects treatment firms differently than control firms. For example, if Reg FD increases management forecast frequency more for treatment firms than for control firms, it can bias against finding results supporting H2 in the positive return subsample. However, this cannot explain the positive and statistically significant *DID_IPT* in the negative return subsample.

Third, the lack of statistically significant results in the positive return subsample may be due to a lack of power in the research design. In particular, the control and treatment portfolios in the subsample, comprising 50 observations each, may be too small to sufficiently average away idiosyncratic returns, which can result in return reversals that can inflate the *IPT* metric. While portfolios with 50 observations noticeably average away return reversals, as observed in appendix B (table B.2, panel B), the maximum *RET_REV* approaches two, which I deem to be problematic for interpreting *IPT* as the timeliness of price discovery. Hence, I interpret these results with caution. Indeed, in figures 4.1 and 4.2, the post-period treatment portfolio *IPT* curves manifest larger return reversals than the other curves, which can bias against finding results supporting H2. In contrast, the negative return subsample portfolios are substantially larger, comprising 174 observations each. Portfolios with 200 observations have considerably lower return reversals than those with 50 observations (see panel B of table B.2).

Finally, it is possible that changes in external channels of information, such as analysts, confound results. For example, analyst following may increase *IPT* from the pre- to the post-period more for derivative non-users than for users, biasing against finding results supporting H2. However, in section 4.3.3.3, I do not find any differential changes in analyst following around SFAS 133 for derivative users and non-users. Hence, I cannot conclude that the lack of evidence supporting H2 relates to changes in analyst following.

In summary, I find support for H1 but not H2. I explain potential reasons for lack of evidence for H2 above. Based on additional analysis, I find some evidence consistent with the confounding effects of Reg FD and an inability to adequately average away the random timing of firm-level returns for the statistically insignificant *DiD_IPT* results in the positive return subsample. In addition, both the null results in the positive return subsample and the opposite results in the negative return subsample could be associated with enhanced transparency of derivative use after SFAS 133, which can improve the confirmability of financial reports.

Chapter 5

Discussion and Conclusion

This thesis studies the confirmatory role of accounting by examining the impact of fair value accounting on two aspects of informational efficiency: the credibility of management forecasts and the timeliness of price discovery. As discussed in chapter 2, there exists an ongoing debate regarding the trade-off between the relevance and reliability or verifiability of fair values, which I argue reflects differential views on the primary role of accounting. Nonetheless, there has been a transition towards greater fair value accounting in the last four decades. Given this shift, standard setters can benefit from understanding the consequences of fair value accounting from the perspective that accounting can enhance the credibility of managers' voluntary disclosures. Specifically, the confirmatory role of financial reports highlights the need to assess the impact of fair value accounting on information outside of the financial reports.

I argue that lower reliability or verifiability associated with fair values can reduce the financial reports' ability to serve a confirmatory role. Thus, I hypothesize that an increase in exposure to fair value accounting reduces the credibility of voluntary disclosures and the timeliness of price discovery. To examine these hypotheses, I exploit SFAS 133 (FASB 1998), which increases fair value accounting exposure for derivative users by mandating all derivatives to be reported at fair value. I compare the credibility of voluntary disclosures and the timeliness of price discovery of derivative users to those of derivative non-users, pre- versus post-mandatory adoption of SFAS 133, using a DiD research design.

Using management forecasts as a key voluntary disclosure, I find results suggesting that an increase in exposure to fair value accounting impairs the credibility of management forecasts, when the forecast conveys good news. However, fair value exposure does not appear to affect the credibility of forecasts that convey bad news. A potential explanation for the lack of results supporting my hypothesis for bad news management forecasts is that the confirmability of financial reports is not as important for management-issued bad news, which is inherently more credible than management-issued good news. These results highlight a potential unintended consequence of fair value accounting on voluntary disclosures. I caveat that these findings are limited to the constant derivative sample and, thus, may not generalize to firms that change their decision to use (not use) derivatives post-SFAS 133. Furthermore, if there remain time-varying differences between control and treatment firms influencing the credibility of management forecasts that I fail to capture, the generalizability of the results may be limited to derivative users (i.e., treatment firms).

In additional analyses, I find that loss forecasts, which comprise less than 2% of my sample, are highly influential. The failure to control for these loss forecast observations leads to null results. In contrast, controlling for these forecasts by including an indicator variable for loss forecasts or excluding them from the sample strengthens results in support of H1 for good news forecasts.

In contrast to H1, I fail to find results supporting the second hypothesis, that an increase in exposure to fair value accounting reduces the timeliness of price discovery. Specifically, in the positive intraperiod return subsample, I find results in the predicted direction, but lacking statistical significance. In the negative intraperiod return subsample, on the other hand, I find results in the opposite direction from that predicted. These results suggest that fair value accounting does not affect or even increases the timeliness of price discovery. To the extent that fair values are credible, fair value accounting can enhance timely price discovery by incorporating more current information into the interim financial reports, relative to historical cost accounting.

I also identify three alternative explanations for these results. First, SFAS 133 may not affect the timeliness of price discovery in the predicted manner due to an increase in the transparency of derivative use. Under SFAS 133, additional requirements for hedge qualification, hedge accounting and derivative-related disclosures led to an improvement in the transparency of derivative use, which can strengthen, rather than weaken, the confirmability of financial reports. Because my research strategy identifies derivative users as the group affected by SFAS 133, it cannot disentangle the effects of increased uncertainties created by fair value accounting from the effects of increased transparency; both are associated with SFAS 133. Future research may find a way to isolate these two effects.

Second, these results may be driven by potential confounding effects of Reg FD, which was enacted between the pre- and post-periods in this thesis. Additional analysis indicates that the frequency of management forecasts increases significantly from the pre- to the post-SFAS 133 period in both treatment and control firms, which is consistent with the effects of Reg FD documented in prior literature (e.g., Heflin et al. 2003; Anilowski et al. 2007; Choi et al. 2010). While I aim to control for temporal changes in the timeliness of price discovery using a DiD research design, it is possible that Reg FD affects treatment firms more strongly than control firms. If so, this can bias against finding results supporting H2.

Finally, a potential explanation for the null results in the positive intraperiod return subsample is that I cannot adequately average away firm-level returns, perhaps due to an insufficient sample size. Specifically, in this subsample, I find that the *IPT* curve of derivative users has a larger return reversal than that of non-users in the post-period, which can bias against finding results supporting the hypothesis.

My study has four primary contributions. First, it contributes to the stream of empirical literature examining the confirmatory role of financial reports. This rather sparse literature (e.g., Beniluz 2005; Ball et al. 2012a; Frankel et al. 2017) focuses on the impact of audit quality or the firm's commitment to financial reporting quality on management forecast characteristics, holding constant accounting standards. I extend this literature by focusing on the impact of a *change* in accounting standard that can adversely affect the confirmability of financial reports. By comparing management forecast credibility between derivative users and non-users around SFAS 133, I find that an accounting standard that increases measurement uncertainty can negatively affect the credibility of voluntary disclosures, consistent with the confirmatory theory. Also, incremental to prior studies in this stream, I examine the impact on the timeliness of price discovery. However, these results suggest that fair value accounting does not impede timely price discovery, contrary to expectations.

Second, by considering the impact of accounting on information outside of financial reports, this study provides policy implications for standard setters. The objective of financial reporting is "to provide financial information about the reporting entity that is useful to existing and potential investors…in making decisions about providing resources to the entity" (SFAC No.8 OB2, FASB 2010). Given that investor decisions are largely influenced by information outside the financial reports, it is important to evaluate whether and, if so, how accounting affects this other information. Ball (2001) stresses the importance of considering the interaction between financial reporting and other disclosures when evaluating the efficiency of a disclosure system as a whole. When we examine the disclosure system as a whole, it becomes evident that the reliability of financial reports serves a critical role in sustaining the integrity of financial information in capital markets. In particular, no other public information source in capital markets is independently verified on a routine basis the way financial reports are. My findings suggest that changes in accounting can have unintended consequences for the quality of information outside of the financial reports by adversely affecting their credibility. Hence, future research assessing the impact of accounting in capital markets should consider that the financial report is only one of many information sources in the public financial reporting and disclosure system.

Third, this study contributes to the literature examining the consequences of fair value accounting in a capital market setting. Much of this literature (e.g., Barth 1994; Eccher et al. 1996; Song et al. 2010) comprises value relevance studies. These studies assess whether the fair values on the financial reports reflect information that is relevant to investors' valuation decisions, but they cannot, nor are they intended to, assess whether the fair values, at the time of the financial report, contain new information content. Illustrating this point, Barth, Beaver and Landsman (2001) state that "accounting information can be value relevant but not decision relevant if it is superseded by more timely information" (p.80). The key distinction between value relevance and decision relevance is, then, timeliness. Thus, I examine an information timeliness construct that captures the impact of various information channels. My findings, subject to potential alternative explanations discussed above, suggest that fair value accounting does not impede timely price discovery. In fact, when the intraperiod return is negative, the findings suggest that fair value accounting may lead to more, not less, timely price discovery.

Fourth, this study contributes to the stream of literature using *IPT* metrics. Prior literature has used both firm-level and portfolio-level *IPT* metrics. Prior studies using portfolio-level *IPT* metrics highlight the importance of averaging away the idiosyncratic timing of news arrival at the firm level for the *IPT* to be useful for making inferences about the timeliness of price discovery. However, these studies do not discuss this issue in detail. I contribute to this literature by demonstrating that the portfolio-level metric averages away the random timing of firm-specific news considerably. Specifically, I show that the impact of return reversals are substantially reduced in portfolio-level *IPT* metrics, relative to firm-level *IPT* metrics, and provide some insight into how portfolio size affects return reversals using simulation analysis. I also create a proxy to capture the extent of return reversals to assess their impact on *IPT*.

My findings and their implications need to be interpreted with caution in light of the caveats discussed above. The thesis is also subject to generalizability limitations. In this thesis, I focus on the impact of SFAS 133, which affects derivative instruments. The fair values for such instruments generally comprise substantial measurement uncertainty and are bi-directional in nature. Fair values that involve little or no measurement uncertainty, such as those for actively traded securities, may not dampen the confirmability of financial reports. Furthermore, the impact of fair value accounting on the confirmability of financial reports may differ when fair value changes are uni-directional, as in the case of goodwill.

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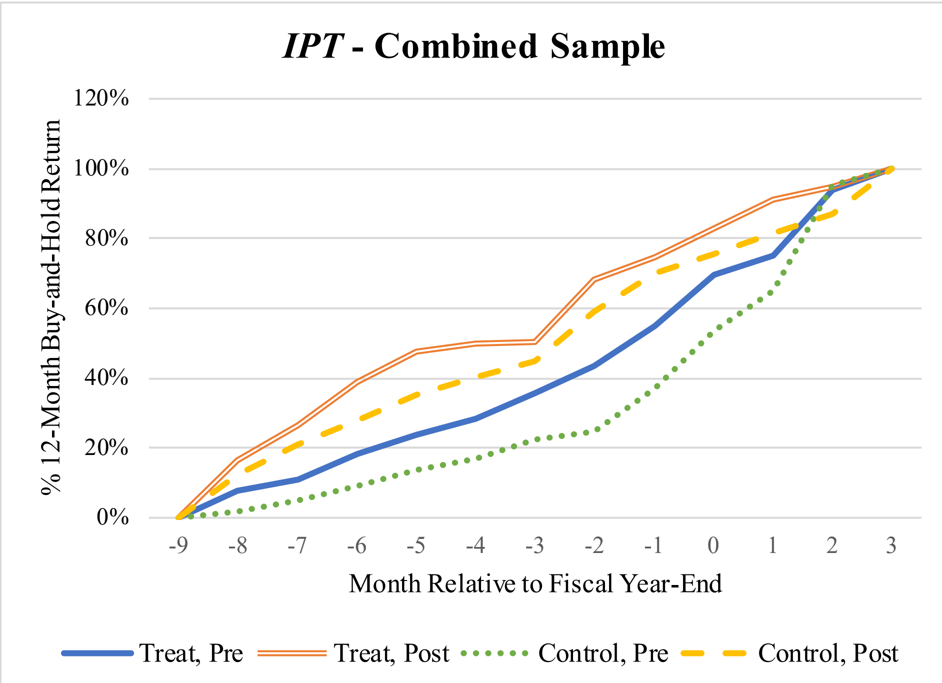
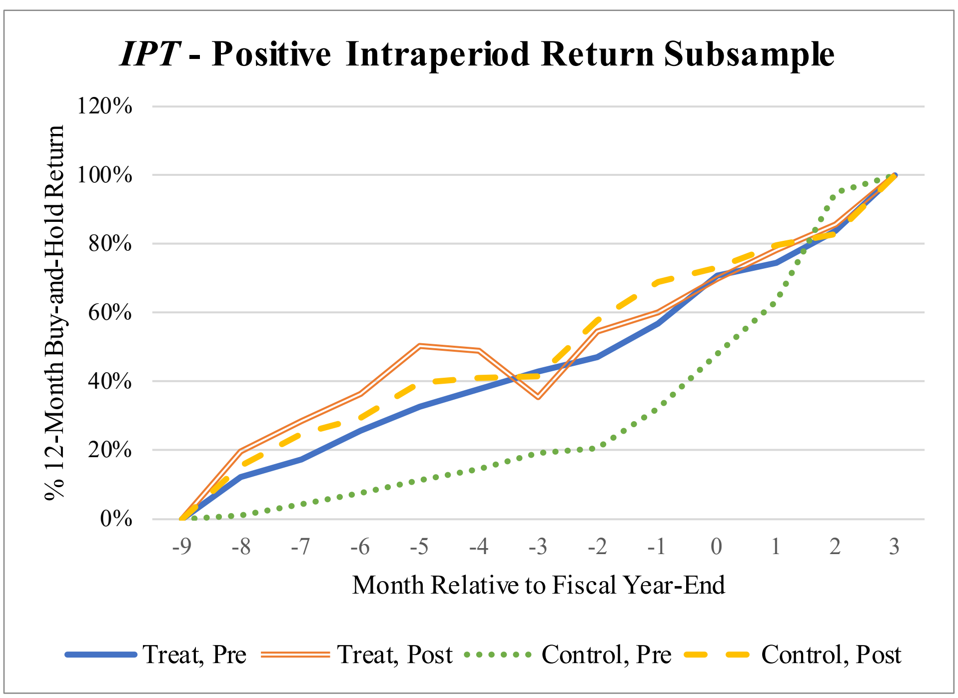
FIGURE 4.1

The Effect of Exposure to Fair Value Accounting on the Timeliness of Price Discovery - IPT Curves in the Unmatched H2 Sample

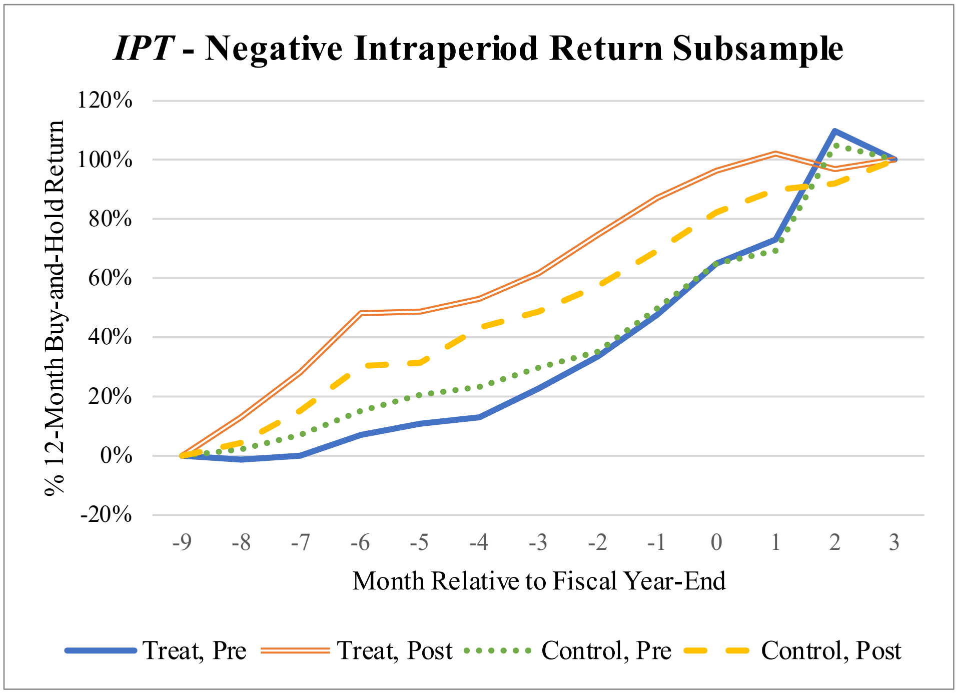
Panel A



Panel B



Panel C

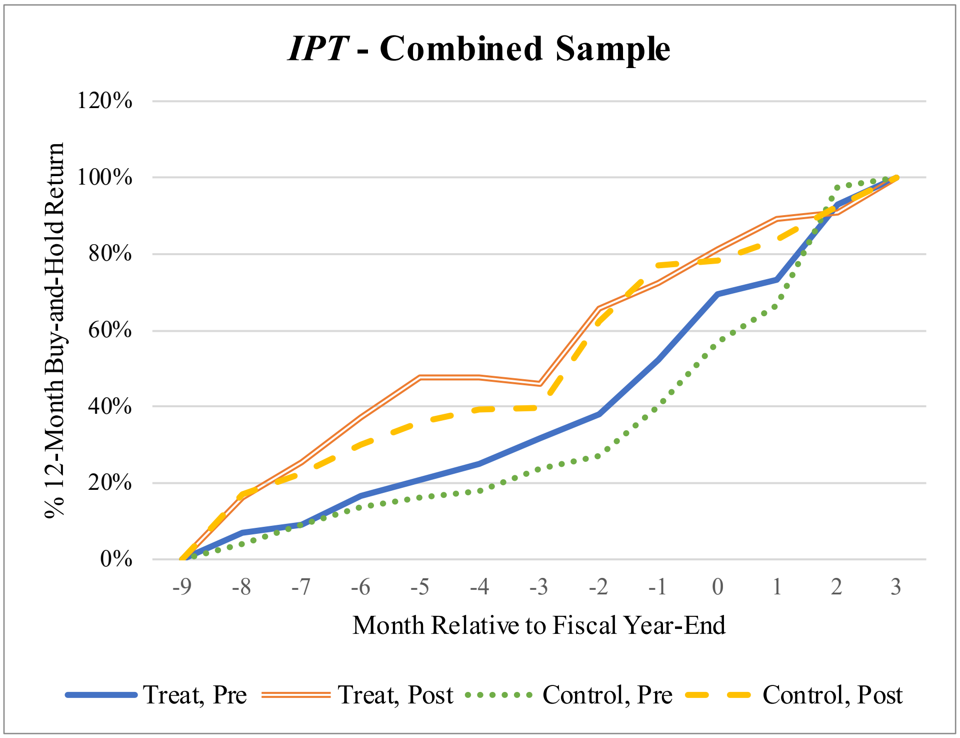


This figure plots the *IPT* curves for unmatched H2 sample (3.4, panel B). Panels A, B and C plot the *IPT* curves for the combined, the positive intraperiod return and the negative intraperiod return sample/subsamples, respectively. The buy-and-hold abnormal return at the end of each month is plotted as a percentage of the 12-month buy-and-hold abnormal return. The portfolio buy-and-hold abnormal return is the equally-weighted hedge return one would earn based on perfect foresight of the 12-month buy-and-hold return. It is calculated as the return one would earn by taking a long position in firms with a positive 12-month buy-and hold return and a short position in firms with a negative 12-month buy-and hold return. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods.

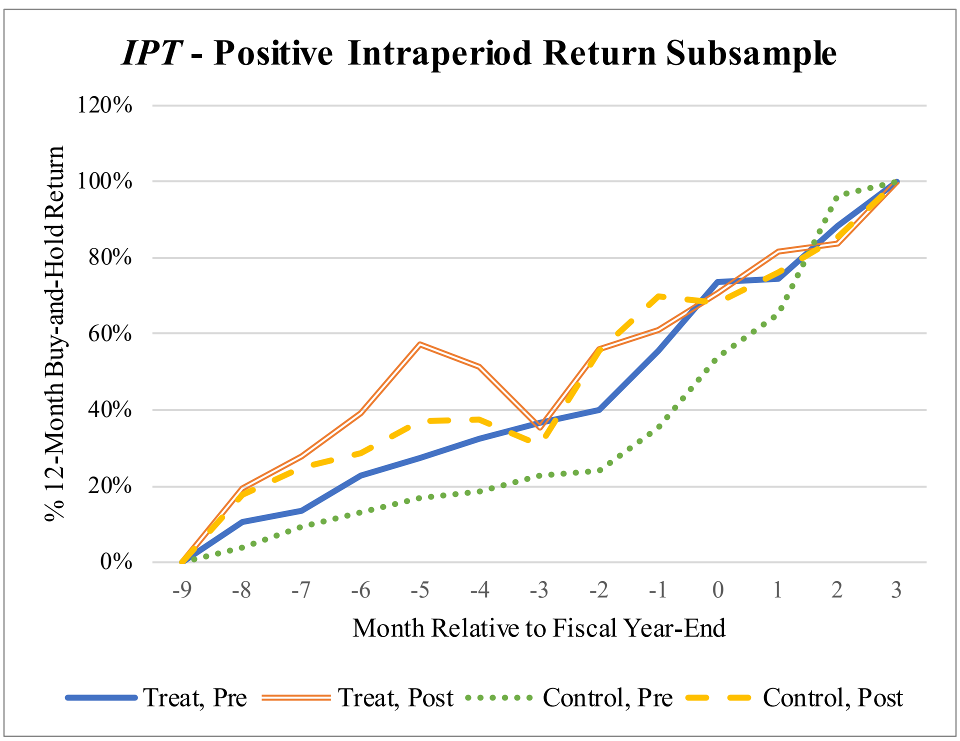
FIGURE 4.2

The Effect of Exposure to Fair Value Accounting on the Timeliness of Price Discovery - IPT Curves in the Matched H2 Sample

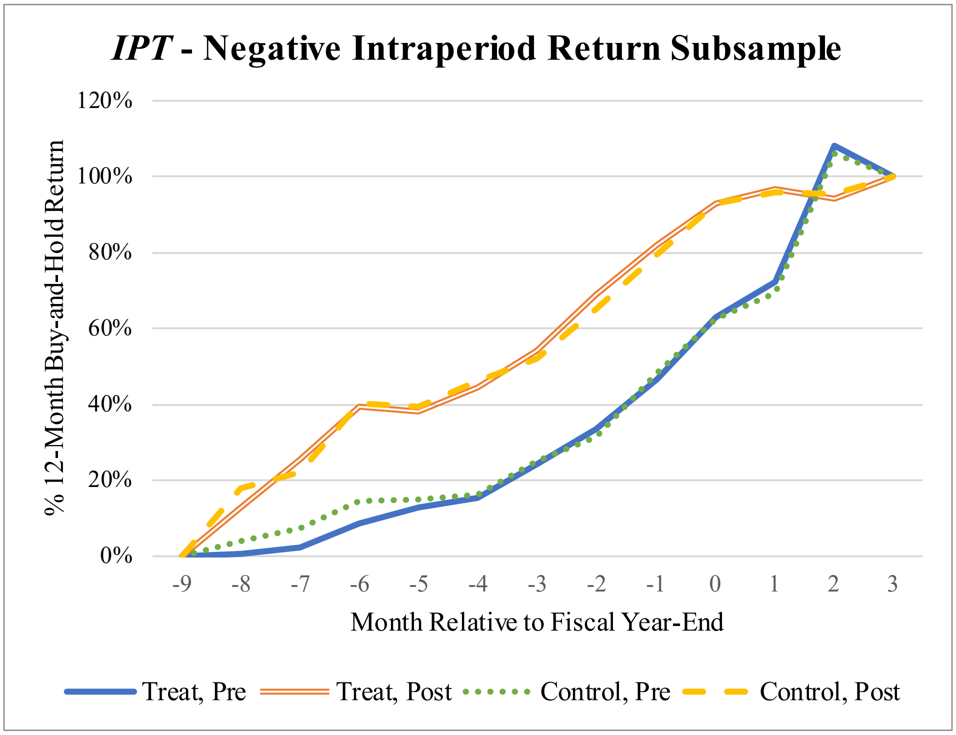
Panel A



Panel B



Panel C

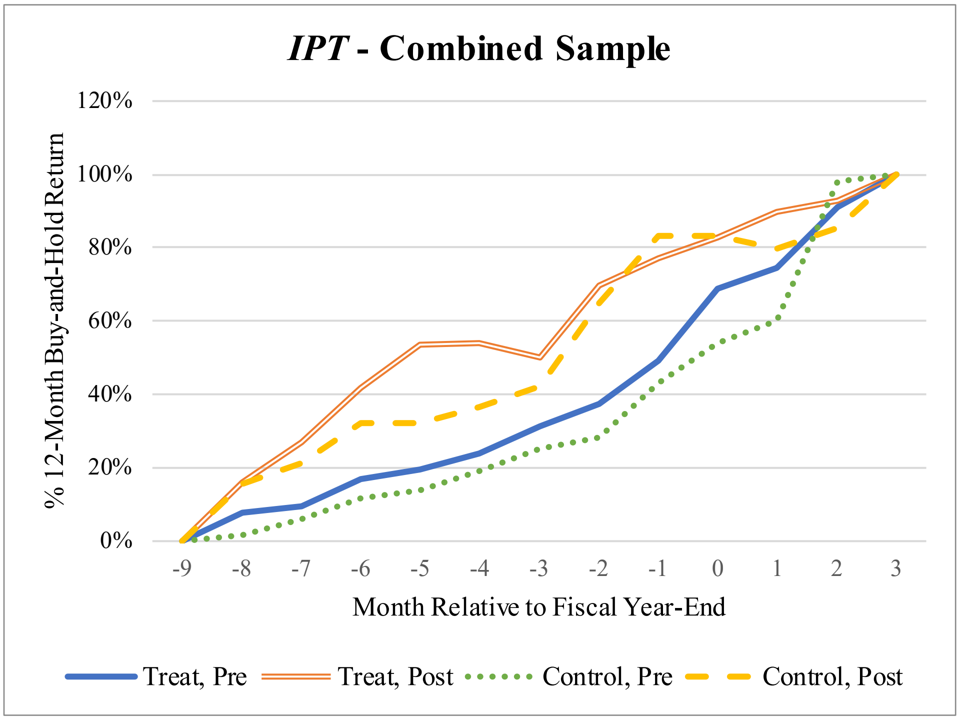


This figure plots the *IPT* curves for matched H2 sample (table 3.4, panel B). Panels A, B and C plot the *IPT* curves for the combined, the positive intraperiod return and the negative intraperiod return sample/subsamples, respectively. The buy-and-hold abnormal return at the end of each month is plotted as a percentage of the 12-month buy-and-hold abnormal return. The portfolio buy-and-hold abnormal return is the equally-weighted hedge return one would earn based on perfect foresight of the 12-month buy-and-hold return. It is calculated as the return one would earn by taking a long position in firms with a positive 12-month buy-and hold return and a short position in firms with a negative 12-month buy-and hold return. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods.

FIGURE 4.3

The Effect of Exposure to Fair Value Accounting on the Timeliness of Price Discovery - IPT Curves in the Constant Derivative Matched Sample

Panel A



Panel B

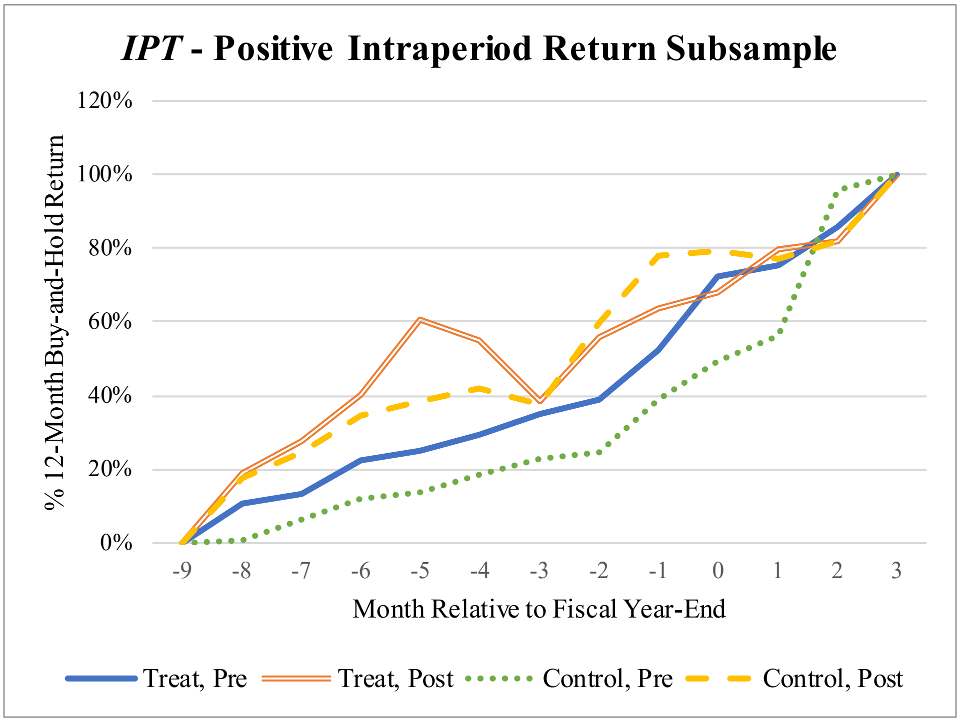
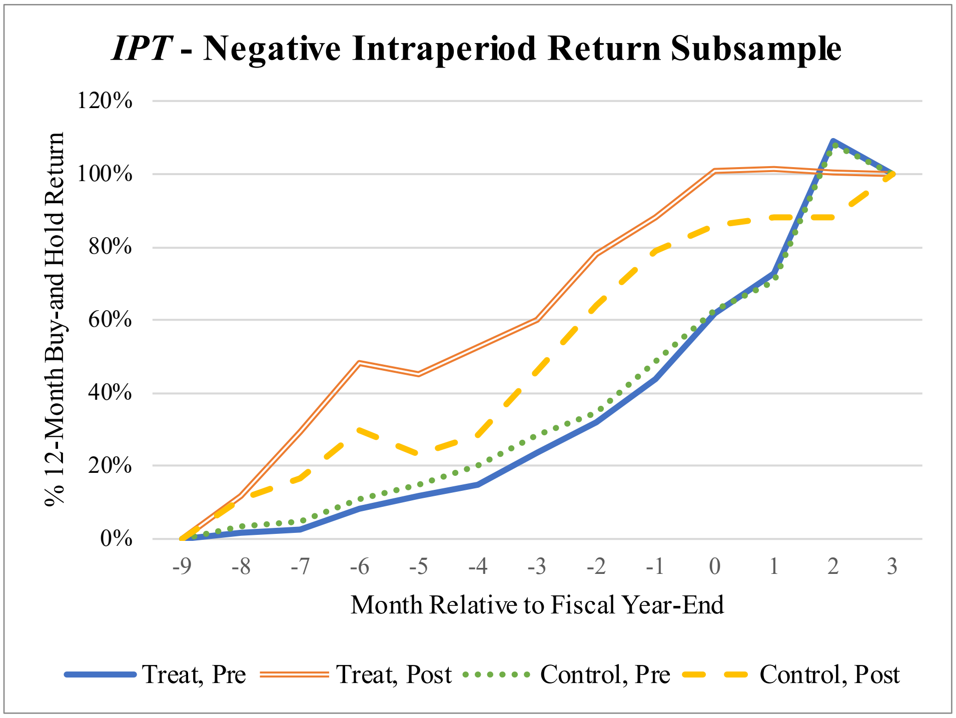


FIGURE 4.3 - *Continued*

Panel C



This figure plots the *IPT* curves for constant derivative matched H2 sample (table 4.19, panel B). Panels A, B and C plot the *IPT* curves for the combined, the positive intraperiod return and the negative intraperiod return sample/subsamples, respectively. The buy-and-hold abnormal return at the end of each month is plotted as a percentage of the 12-month buy-and-hold abnormal returns. The portfolio buy-and-hold abnormal return is the equally-weighted hedge return one would earn based on perfect foresight of the 12-month buy-and-hold return. It is calculated as the return one would earn by taking a long position in firms with a positive 12-month buy-and hold return and a short position in firms with a negative 12-month buy-and hold return. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods.

TABLE 3.1

Sample Selection – H1

Panel A: Set of Observations with Necessary Data

U.S. firms (Fiscal years 1998, 1999, 2001 and 2002)	Number of MFs	Number of Firm-Years	Number of Firms
Intersection of U.S. firms on Compustat and CRSP	9,325	26,455	8,883
Less: Observations in financial industry	(1,029)	(6,951)	(2,221)
Less: Observations with no annual earnings per share (EPS) management forecasts (MFs) on IBES	-	(16,078)	(4,744)
Set of annual EPS MF issuers on IBES	8,296	3,426	1,918
Less:			
Observations other than point or closed range estimates	(681)	(274)	(134)
Observations issued on or after the fiscal year-end forecasted	(412)	(196)	(100)
Observations issued before the prior year's earnings announcement	(1,084)	(264)	(102)
Observations with incomplete data to calculate $MF\_CAR_{0,1}$	(70)	(33)	(22)
Observations with no analyst forecast for the corresponding firm year issued prior to the MF date	(207)	(114)	(71)
Observations with no analyst forecast for the corresponding firm year issued within (-2,-90) days of the MF date	(656)	(175)	(86)
Observations with pre-MF share price below $1.00	(11)	(7)	(5)
Observations where the absolute value of MF_SURP is greater than 0.10	(69)	(39)	(26)
Observations with incomplete data to construct control variables	(119)	(70)	(47)
Multiple observations for each firm year, keeping only the latest MF	(2,733)	-	-
Set of observations with necessary management forecast- and firm-level data	2,254	2,254	1,325

Continued on next page

TABLE 3.1 - *Continued*

Panel B: Unmatched and Matched Samples

	Number of Firm-Years	Number of Firms
Set of observations with necessary management forecast- and firm-level data	2,254	1,325
Less: Firms with no pre-period observation	(1,175)	(832)
Less: Firms with no post-period observation	(225)	(199)
Set of available observations prior to identifying treatment firms and matching	854	294
Less: Firms missing SEC 10-k filing to identify treatment firms in latest pre-period	(26)	(10)
Unmatched sample	828	284
Less: Firms with no matches using CEM on *OCFVOL, MVE*, industry and fiscal year	(510)	(172)
Matched sample	318	112

Panel C: Samples, by Treatment versus Control

	Number of Firm-Years		Number of Firms	
Sample	Control	Treat	Control	Treat
Unmatched sample	263	565	97	187
Matched sample	153	165	56	56

This table reports the sample selection process for the H1 samples. The sample period includes fiscal years 1998, 1999, 2001 and 2002, where year t represents fiscal years ending June t to May t+1 to correspond with the effective date of SFAS 133. Panel A reports the sample selection process to arrive at the set of observations with necessary management forecast- and firm-level data. Panel B reports the sample selection process to identify the unmatched and the matched samples. Panel C reports each of the unmatched and matched samples, by treatment versus control group. Treatment firms include derivative users and control firms include derivative non-users, identified in the latest pre-SFAS 133 period year (1998 or 1999). The matched sample is identified using CEM on *OCFVOL* (5 cutpoints), *MVE* (4 cutpoints), the Fama-French 12 industry classifications and fiscal year. All variables are defined in Appendix A.

TABLE 3.2

Covariate Balance - H1

Panel A: Before Matching (Unmatched Sample)

	Control (N = 97)		Treatment (N = 187)		Difference in Means	
Variable	Mean	St. Dev.	Mean	St. Dev.	Diff.	T-stat
OCFVOL	0.091	0.110	0.041	0.034	-0.049	-4.29 ***
MVE	6.430	1.607	8.052	1.722	1.622	7.87 ***

Panel B: After Matching (Matched Sample)

	Control (N = 56)		Treatment (N = 56)		Difference in Means	
Variable	Mean	St. Dev.	Mean	St. Dev.	Diff.	T-stat
OCFVOL	0.054	0.041	0.046	0.032	-0.008	-1.19
MVE	6.974	1.633	7.331	1.721	0.357	1.13

This table reports the difference in the covariate means between the treatment and control groups for the H1 sample in the latest pre-period year, the year of the match, before and after matching. The matched sample is identified using CEM on *OCFVOL* (5 cutpoints), *MVE* (4 cutpoints), the Fama-French 12 industry classifications and fiscal year (see panel B of table 3.1). ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the two-tailed tests. All variables are defined in Appendix A.

TABLE 3.3

Sample Representativeness - H1

Panel A: Temporal Distribution of Samples versus Population

	Matched Sample		Unmatched Sample		Set of Annual EPS MF Issuers		Compustat Population	
Fiscal year	N	% Sample	N	% Sample	N	% Group	N	% Pop.
1998	71	22.3	167	20.2	283	12.4	5,566	28.5
1999	70	22.0	189	22.8	322	14.1	5,281	27.1
2001	84	26.4	225	27.2	807	35.4	4,511	23.1
2002	93	29.3	247	29.8	868	38.1	4,146	21.3
Total	318	100.0	828	100.0	2,280	100.0	19,504	100.0

Continued on next page

TABLE 3.3 - *Continued*

Panel B: Industry Distribution of Samples versus Population

	Matched Sample		Unmatched Sample		Set of Annual EPS MF Issuers		Compustat Population	
Industry	N	% Sample	N	% Sample	N	% Set	N	% Pop.
Consumer Non-Durables	41	12.9	97	11.7	286	8.4	1,218	6.2
Consumer Durables	10	3.1	27	3.3	117	3.4	565	2.9
Manufacturing	20	6.3	114	13.8	413	12.1	2,284	11.7
Energy and Extraction	5	1.6	10	1.2	83	2.4	742	3.8
Chemicals and Allied Products	11	3.5	42	5.1	100	2.9	472	2.4
Business Equipment	52	16.3	113	13.6	707	20.6	4,966	25.5
Telecommunications	14	4.4	23	2.8	87	2.5	693	3.6
Utilities	-	0.0	51	6.2	186	5.4	569	2.9
Wholesale and Retail	72	22.6	142	17.1	557	16.3	2,367	12.1
Healthcare	27	8.5	101	12.2	347	10.1	2,517	12.9
Other	66	20.8	108	13.0	543	15.9	3,111	16.0
Total	318	100.0	828	100.0	3,426	100.0	19,504	100.0

This table reports the sample representativeness of the unmatched and matched H1 samples (see table 3.1, panel B) in relation to the Compustat population and the set of annual EPS MF issuers (see table 3.1, panel A). Panels A and B of this table provide the temporal and industry distributions, respectively, of each of the samples, set and population. All samples/set/population include fiscal years 1998, 1999, 2001 and 2002 and excludes firms in the financial industry. Industry classifications are based on the Fama and French 12 industry classifications.

TABLE 3.4
Sample Selection - H2

Panel A: Set of Observations with Necessary IPT Data

	Number of Firm-Years			Number of Firms
	Fiscal 1999	Fiscal 2001	Total	Total
Intersection of U.S. firms on Compustat and CRSP	7,107	6,182	13,289	7,765
Less:				
Observations in financial industry	(1,826)	(1,671)	(3,497)	(1,953)
Observations with less than 10 months of monthly returns for fiscal year	(762)	(404)	(1,166)	(601)
Set of observations with necessary IPT data	4,519	4,107	8,626	5,211

Panel B: Unmatched and Matched Samples

	Number of Firm-Years	Number of Firms
Set of observations with necessary IPT data	8,626	5,211
Less: Firms with no pre-period observation	(692)	(692)
Less: Firms with no post-period observation	(1,104)	(1,104)
Set of firms with both pre- and post-period observations	6,830	3,415
Less: Firms with insufficient data to construct *OCFVOL* in pre-period	(172)	(86)
Less: Firms missing *MVE* in pre-period	(134)	(67)
Less: Firms with differing signs of intraperiod news in pre- and post-periods	(3,712)	(1,856)
Set of available observations prior to identifying treatment firms and matching	2,812	1,406
Less: Firms missing SEC 10-k filing to identify treatment firms in pre-period	(58)	(29)
Unmatched sample	2,754	1,377
Less: Firms with no matches using CEM on *OCFVOL*, *MVE*, industry and sign of intraperiod news	(1,578)	(789)
Matched sample	1,176	588

Continued on next page

TABLE 3.4 - *Continued*

Panel C: Samples, by Treatment versus Control

	Number of Firm-Years		Number of Firms	
Sample/Subsample	Control	Treat	Control	Treat
Unmatched sample	1,770	984	885	492
Matched sample	588	588	294	294
Positive intraperiod return matched subsample	132	132	66	66
Negative intraperiod return matched subsample	456	456	228	228

This table reports the sample selection process for H2. The sample period includes fiscal years 1999 and 2001, where year t represents fiscal years ending June t to May t+1 to correspond with the effective date of SFAS 133. Panel A reports the sample selection process to arrive at the set of observations with necessary IPT data. Panel B reports the sample selection process to identify the unmatched and matched samples. Panel C reports the unmatched and matched samples, by treatment and control group. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period (1999). The positive (negative) intraperiod return subsample includes observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. The matched sample is identified using CEM on *OCFVOL* (28 cutpoints), *MVE* (8 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return. All variables are defined in Appendix A.

TABLE 3.5

Sample Representativeness - H2

Panel A: Industry Distribution of Samples versus Population

	Matched Sample		Unmatched Sample		Compustat Population			
					1999		2001	
Industry	N	% Sample	N	% Sample	N	% Pop.	N	% Pop.
Consumer Non-Durables	44	7.5	86	6.2	340	6.4	262	5.8
Consumer Durables	12	2.0	42	3.1	149	2.8	125	2.8
Manufacturing	114	19.4	201	14.6	608	11.5	520	11.5
Energy and Extraction	34	5.8	69	5.0	189	3.6	174	3.9
Chemicals and Allied Products	12	2.0	40	2.9	127	2.4	106	2.4
Business Equipment	98	16.7	292	21.2	1373	26.0	1220	27.1
Telecommunications	6	1.0	23	1.7	202	3.8	170	3.8
Utilities	24	4.1	57	4.1	153	2.9	125	2.8
Wholesale and Retail	82	13.9	153	11.1	663	12.6	513	11.4
Healthcare	52	8.8	199	14.5	622	11.8	612	13.6
Other	110	18.7	215	15.6	855	16.2	684	15.2
Total	588	100.0	1,377	100.0	5,281	100.0	4,511	100.0

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TABLE 3.5 – *Continued*

Panel B: Means of Confounding Variables in Samples versus Population

Pre-period (fiscal 1999)	Matched Sample			Unmatched Sample			Compustat Population	
Variable	N	Mean		N	Mean		N	Mean
OCFVOL	588	0.061	***	1,377	0.091	*	4,646	0.098
MVE	588	5.461	***	1,377	5.128	***	4,818	4.980
ANALYSTS_N	588	1.577	***	1,377	1.384		5,281	1.352
MF_N	588	0.326	***	1,377	0.280	***	5,281	0.229

Post-period (fiscal 2001)	Matched Sample			Unmatched Sample			Compustat Population	
Variable	N	Mean		N	Mean		N	Mean
OCFVOL	588	0.059	***	1,377	0.082	***	4,292	0.125
MVE	588	5.337		1,377	4.905	***	4,408	5.169
ANALYSTS_N	588	1.356		1,377	1.223	***	4,511	1.332
MF_N	588	0.562		1,377	0.495	*	4,511	0.527

This table reports the sample representativeness of the H2 samples (table 3.4, panel B) in relation to the Compustat population. Panel A of this table provides the industry distributions of the unmatched and matched samples and the Compustat population for each of the pre- and post-periods. Industry classifications are based on the Fama and French 12 industry classifications. Panel B of this table provides the mean descriptive statistics for the unmatched and matched samples and the Compustat population for each of the pre- and post-periods and tests whether the sample mean is equal to the mean value of the H2 population. Both the unmatched and matched samples and the Compustat population include fiscal years 1999 and 2001 and excludes firms in the financial industry. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the two-tailed t-tests comparing the means of the confounding variables between the matched (unmatched) sample and the Compustat population. All variables are defined in Appendix A.

TABLE 4.1

Industry Distribution for H1

	Unmatched Sample				Matched Sample			
	All		*Treatment*		*All*		*Treatment*	
Industry	N	% Sample	N	% Industry	N	% Sample	N	% Industry
Consumer Non-Durables	33	11.6	26	78.8	14	12.5	7	50.0
Consumer Durables	10	3.5	7	70.0	4	3.6	2	50.0
Manufacturing	39	13.7	35	89.7	8	7.1	4	50.0
Energy and Extraction	4	1.4	3	75.0	2	1.8	1	50.0
Chemicals and Allied Products	13	4.6	11	84.6	4	3.6	2	50.0
Business Equipment	39	13.7	21	53.8	18	16.1	9	50.0
Telecommunications	7	2.5	2	28.6	4	3.6	2	50.0
Utilities	16	5.6	15	93.8	0	0.0	0	NA
Wholesale and Retail	50	17.6	24	48.0	24	21.4	12	50.0
Healthcare	34	12.0	25	73.5	10	8.9	5	50.0
Other	39	13.7	18	46.2	24	21.4	12	50.0
Total	284	100.0	187	65.8	112	100.0	56	50.0

This table reports the industry distribution of the unmatched and matched H1 samples (table 3.1, panel B), using unique firm observations. Treatment firms include derivative users identified in the latest pre-period. Industry classifications are based on the Fama and French 12 industry classifications.

TABLE 4.2

Descriptive Statistics – H1

Panel A: Unmatched Sample

Variables	N	Mean	Std. Dev.	P10	P25	P50	P75	P90
$MF\_CAR_{0,1}$	828	-0.015	0.097	-0.121	-0.045	-0.006	0.030	0.072
MF_SURP	828	-0.003	0.012	-0.011	-0.003	0.000	0.001	0.003
MF_BNEWS	828	0.550	0.498	0	0	1	1	1
OCFVOL	828	0.049	0.051	0.013	0.021	0.035	0.060	0.096
$MF\_SURP \times \lvert MF\_SURP \rvert$	828	0.000	0.001	0.000	0.000	0.000	0.000	0.000
MF_LOSS	828	0.017	0.129	0	0	0	0	0
MF_WIDTH	828	0.002	0.004	0.000	0.000	0.001	0.003	0.006
MF_HORIZON	828	133.694	95.330	23	66	98	193.5	284
MTB	828	4.462	16.299	1.049	1.690	2.744	4.846	8.480
MVE ($B)	828	11.765	31.064	0.193	0.571	1.968	7.225	24.749
EA_CONCUR	828	0.530	0.499	0	0	1	1	1
EA_SURP	439	0.003	0.040	-0.018	-0.004	0.002	0.005	0.014
$EA\_SURP \times \lvert EA\_SURP \rvert$	439	0.001	0.009	0.000	0.000	0.000	0.000	0.000
EA_LOSS	439	0.105	0.307	0	0	0	0	1

Panel B: Matched Sample

Variables	N	Mean	Std. Dev.	P10	P25	P50	P75	P90
$MF\_CAR_{0,1}$	318	-0.016	0.101	-0.114	-0.044	-0.006	0.038	0.073
MF_SURP	318	-0.002	0.011	-0.010	-0.002	0.000	0.001	0.004
MF_BNEWS	318	0.528	0.500	0	0	1	1	1
OCFVOL	318	0.048	0.034	0.014	0.024	0.038	0.061	0.096
$MF\_SURP \times \lvert MF\_SURP \rvert$	318	0.000	0.001	0.000	0.000	0.000	0.000	0.000
MF_LOSS	318	0.006	0.079	0	0	0	0	0
MF_WIDTH	318	0.002	0.004	0.000	0.000	0.001	0.003	0.005
MF_HORIZON	318	135.692	97.752	22	67	93.5	223	282
MTB	318	4.153	18.042	0.908	1.502	2.649	4.277	6.498
MVE ($B)	318	8.629	26.755	0.181	0.468	1.428	4.613	15.748
EA_CONCUR	318	0.538	0.499	0	0	1	1	1
EA_SURP	171	0.001	0.025	-0.018	-0.002	0.002	0.006	0.011
$EA\_SURP \times \lvert EA\_SURP \rvert$	171	0.000	0.004	0.000	0.000	0.000	0.000	0.000
EA_LOSS	171	0.094	0.292	0	0	0	0	0

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TABLE 4.2 - *Continued*

Panel C.1: Combined Unmatched Sample, Control versus Treatment Firms

Variables	Control N	Control Mean	Control Std. Dev.	Treat N	Treat Mean	Treat Std. Dev.	Difference in Means Diff.	Difference in Means T-stat
MF_SURP	263	-0.004	0.015	565	-0.002	0.011	0.003	2.45 *
MF_BNEWS	263	0.536	0.500	565	0.556	0.497	0.020	0.53
OCFVOL	263	0.068	0.075	565	0.040	0.031	-0.028	-5.77 ***
MF_SURP × |*MF_SURP*|	263	-0.0002	0.0010	565	0.0000	0.0005	0.0002	2.77 **
MF_LOSS	263	0.042	0.201	565	0.005	0.073	-0.037	-2.87 **
MF_WIDTH	263	0.003	0.005	565	0.002	0.004	-0.001	-2.00 *
MF_HORIZON	263	4.618	0.954	565	4.522	0.967	-0.095	-1.33
MTB	263	3.947	7.901	565	4.701	18.983	0.755	0.81
MVE	263	6.650	1.692	565	8.130	1.742	1.480	11.61 ***
EA_CONCUR	263	0.494	0.501	565	0.547	0.498	0.053	1.41
EA_SURP [a]	130	0.006	0.045	309	0.001	0.038	-0.005	-1.19
EA_SURP × |*EA_SURP*|	130	0.001	0.009	309	0.000	0.009	-0.001	-1.05
EA_LOSS	130	0.115	0.321	309	0.100	0.301	-0.015	-0.46

Panel C.2: Good News Unmatched Subsample, Control versus Treatment Firms

Variables	Control N	Control Mean	Control Std. Dev.	Treat N	Treat Mean	Treat Std. Dev.	Difference in Means Diff.	Difference in Means T-stat
MF_SURP	122	0.003	0.004	251	0.003	0.008	0.001	1.29
OCFVOL	122	0.069	0.076	251	0.042	0.032	-0.027	-3.70 ***
MF_SURP × |*MF_SURP*|	122	0.0000	0.0001	251	0.0001	0.0004	0.0001	2.03 *
MF_LOSS	122	0.041	0.199	251	0.000	0.000	-0.041	-2.27 *
MF_WIDTH	122	0.002	0.005	251	0.002	0.003	-0.001	-1.19
MF_HORIZON	122	4.727	0.868	251	4.608	0.938	-0.119	-1.21
MTB	122	3.532	3.580	251	4.064	14.290	0.532	0.56
MVE	122	6.710	1.767	251	12.850	7.987	1.778	6.54 ***
EA_CONCUR	122	0.549	0.500	251	0.566	0.497	0.017	0.30
EA_SURP [a]	67	0.007	0.045	142	0.000	0.033	-0.007	-1.10
EA_SURP × |*EA_SURP*|	67	0.001	0.008	142	-0.001	0.007	-0.002	-1.32
EA_LOSS	67	0.119	0.327	142	0.092	0.289	-0.028	-0.60

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TABLE 4.2 - *Continued*

Panel C.3: Bad News Unmatched Subsample, Control versus Treatment Firms

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
MF_SURP	141	-0.010	0.019	314	-0.006	0.011	0.004	2.63 **
OCFVOL	141	0.067	0.074	314	0.039	0.031	-0.029	-4.41 ***
MF_SURP × |*MF_SURP*|	141	-0.0004	0.0013	314	-0.0001	0.0006	0.0003	2.60 *
MF_LOSS	141	0.043	0.203	314	0.010	0.097	-0.033	-1.84
MF_WIDTH	141	0.003	0.005	314	0.002	0.004	-0.001	-1.67
MF_HORIZON	141	4.523	1.016	314	4.454	0.985	-0.069	-0.67
MTB	141	4.306	10.270	314	5.211	22.037	0.905	0.6
MVE	141	6.598	1.628	314	8.244	1.707	1.646	9.82 ***
EA_CONCUR	141	0.447	0.499	314	0.532	0.500	0.085	1.68
EA_SURP [a]	63	0.006	0.045	167	0.002	0.042	-0.004	-0.61
EA_SURP × |*EA_SURP*|	63	0.002	0.011	167	0.001	0.010	-0.001	-0.43
EA_LOSS	63	0.111	0.317	167	0.108	0.311	-0.003	-0.07

Panel D.1: Combined Matched Sample, Control versus Treatment Firms

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
MF_SURP	153	-0.002	0.012	165	-0.001	0.010	0.002	1.31
MF_BNEWS	153	0.529	0.501	165	0.527	0.501	-0.002	-0.04
OCFVOL	153	0.051	0.036	165	0.045	0.031	-0.006	-1.54
MF_SURP × |*MF_SURP*|	153	-0.0001	0.0008	165	0.0000	0.0004	0.0001	1.98 *
MF_LOSS	153	0.013	0.114	165	0.000	0.000	-0.013	-1.42
MF_WIDTH	153	0.002	0.005	165	0.002	0.003	0.000	-0.85
MF_HORIZON	153	4.554	1.017	165	4.541	1.005	-0.013	-0.11
MTB	153	4.363	9.957	165	3.959	23.178	-0.404	-0.20
MVE	153	7.185	1.740	165	10.841	7.489	1.781	1.54
EA_CONCUR	153	0.497	0.502	165	0.576	0.496	0.079	1.41
EA_SURP [a]	76	0.002	0.032	95	0.000	0.018	-0.002	-0.54
EA_SURP × |*EA_SURP*|	76	0.000	0.006	95	0.000	0.001	0.000	0.57
EA_LOSS	76	0.092	0.291	95	0.095	0.294	0.003	0.06

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TABLE 4.2 - *Continued*

Panel D.2: Good News Matched Sample, Control versus Treatment Firms

Variables	Control N	Control Mean	Control Std. Dev.	Treat N	Treat Mean	Treat Std. Dev.	Difference in Means Diff.	Difference in Means T-stat
MF_SURP	72	0.002	0.004	78	0.004	0.010	0.002	1.61
OCFVOL	72	0.052	0.037	78	0.052	0.036	0.000	0.05
MF_SURP × |*MF_SURP*|	72	0.0000	0.0001	78	0.0001	0.0005	0.0001	1.70
MF_LOSS	72	0.014	0.118	78	0.000	0.000	-0.014	-1.00
MF_WIDTH	72	0.002	0.004	78	0.002	0.004	0.000	-0.15
MF_HORIZON	72	4.681	0.888	78	4.615	0.950	-0.066	-0.44
MTB	72	3.562	3.605	78	2.367	21.256	-1.195	-0.49
MVE	72	7.197	1.751	78	7.162	1.694	-0.035	-0.12
EA_CONCUR	72	0.583	0.496	78	0.564	0.499	-0.019	-0.24
EA_SURP [a]	42	0.005	0.042	44	0.005	0.017	0.000	0.01
EA_SURP × |*EA_SURP*|	42	0.001	0.008	44	0.000	0.001	0.000	0.38
EA_LOSS	42	0.095	0.297	44	0.068	0.255	-0.027	-0.45

Panel D.3: Bad News Matched Sample, Control versus Treatment Firms

Variables	Control N	Control Mean	Control Std. Dev.	Treat N	Treat Mean	Treat Std. Dev.	Difference in Means Diff.	Difference in Means T-stat
MF_SURP	81	-0.007	0.015	87	-0.006	0.007	0.001	0.71
OCFVOL	81	0.050	0.035	87	0.039	0.026	-0.011	-2.39 *
MF_SURP × |*MF_SURP*|	81	-0.0003	0.0011	87	-0.0001	0.0002	0.0002	1.47
MF_LOSS	81	0.012	0.111	87	0.000	0.000	-0.012	-1.00
MF_WIDTH	81	0.003	0.005	87	0.002	0.003	-0.001	-1.03
MF_HORIZON	81	4.440	1.113	87	4.475	1.052	0.034	0.21
MTB	81	5.074	13.256	87	5.386	24.811	0.312	0.10
MVE	81	7.174	1.741	87	7.782	1.815	0.608	2.21 *
EA_CONCUR	81	0.420	0.497	87	0.586	0.495	0.166	2.17 *
EA_SURP [a]	34	-0.001	0.012	51	-0.004	0.017	-0.003	-1.06
EA_SURP × |*EA_SURP*|	34	0.000	0.000	51	0.000	0.001	0.000	-1.58
EA_LOSS	34	0.088	0.288	51	0.118	0.325	0.029	0.44

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This table reports the descriptive statistics of management forecast and firm characteristic variables for the H1 samples (table 3.1, panel B). Panels A and B report the descriptive statistics for the unmatched and matched samples, respectively. For ease of interpretation, the summary statistics for *EA_SURP, EA_SURP*× |*EA_SURP*| and *EA_LOSS* are based only on management forecasts that are issued concurrently with an earnings announcement. *MF_HORIZON* and *MVE($B)* are reported as unlogged amounts in panels A and B only. Panels C.1 to D.3 report the descriptive statistics of management forecast and firm characteristic variables, by control and treatment firms, as well as the results of the t-test of means for the unmatched and matched samples/subsamples. Panels C.1, C.2 and C.3 report the descriptive statistics and the t-test results for the combined, good news (positive *MF_SURP*), and bad news (negative *MF_SURP*) unmatched sample/subsamples, respectively. Panels D.1, D.2 and D.3 report the descriptive statistics and the t-test results for the combined, good news, and bad news matched sample/subsamples, respectively. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the two-tailed tests. All variables are defined in Appendix A.

TABLE 4.3

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts (Test of H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$				
	Predicted	Unmatched Sample			Matched Sample	
Variable	Sign	(1)	(2)	(3)	(4)	(5)
TREAT	?	-0.007	-0.009	-0.005	-0.032 *	-0.031 *
		[-0.71]	[-0.95]	[-0.49]	[-1.95]	[-1.90]
POST	?	0.031 ***	0.032 ***	0.033 ***	0.035 **	0.034 **
		[2.85]	[2.92]	[2.91]	[2.59]	[2.54]
TREAT × *POST*	?	-0.002	-0.002	-0.003	0.016	0.015
		[-0.12]	[-0.12]	[-0.23]	[0.80]	[0.76]
MF_SURP	+	1.782 **	2.647 **	4.778 ***	1.706 ***	2.489 **
		[1.86]	[1.76]	[3.12]	[4.85]	[2.57]
TREAT × *MF_SURP*	?	-0.244	-0.484	-0.250	0.186	0.028
		[-0.21]	[-0.40]	[-0.28]	[0.14]	[0.02]
POST × *MF_SURP*	+	0.685	0.593	0.272	2.314 ***	2.609 ***
		[0.72]	[0.60]	[0.27]	[4.58]	[3.80]
TREAT × *POST* × *MF_SURP*	-	2.506 *	2.604 *	1.652	-0.317	-0.688
		[1.58]	[1.60]	[1.01]	[-0.18]	[-0.36]
HiOCFVOL	?		-0.016 **	-0.015 **		
			[-2.51]	[-2.41]		
HiOCFVOL × *MF_SURP*	-		-1.070	-1.697 *		
			[-1.07]	[-1.51]		
MF_SURP × |*MF_SURP*|	-			-50.160 ***		-16.430
				[-2.62]		[-1.06]
MF_LOSS	?			0.012		
				[0.40]		
MF_LOSS × *MF_SURP*	-			1.011		
				[0.60]		
HiMF_WIDTH	?			-0.010		
				[-1.36]		
HiMF_WIDTH × *MF_SURP*	-			1.769 **		
				[2.46]		
HiMVE	?			-0.008		
				[-1.19]		
HiMVE × *MF_SURP*	-			-1.554 *		
				[-1.50]		
Constant	?	-0.020 **	-0.012	-0.005	-0.019 *	-0.018 *
		[-2.48]	[-1.32]	[-0.52]	[-1.77]	[-1.73]
N		828	828	828	318	318
Adjusted R^2		0.132	0.138	0.167	0.166	0.165
df_m		7	9	16	7	8
df_r		283	283	283	111	111

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TABLE 4.3 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*						
		DV: $MF\_CAR_{0,1}$				
	Predicted	Unmatched Sample			Matched Sample	
Variable	Sign	(1)	(2)	(3)	(4)	(5)
TREAT	?	-0.005	-0.009	-0.012	-0.037 *	-0.043 *
		[-0.46]	[-0.74]	[-1.00]	[-1.83]	[-1.86]
POST	?	0.031 **	0.029 **	0.026 **	0.029 *	0.028 *
		[2.47]	[2.29]	[2.11]	[1.90]	[1.86]
TREAT × *POST*	?	-0.012	-0.009	-0.007	0.026	0.030
		[-0.76]	[-0.62]	[-0.50]	[1.11]	[1.19]
MF_SURP_GNEWS	+	3.007	3.389	2.160	0.170	0.101
		[0.83]	[0.83]	[0.54]	[0.10]	[0.06]
MF_SURP_BNEWS	+	1.702 **	1.798	5.174 ***	1.898 ***	0.209
		[1.67]	[1.13]	[2.71]	[6.15]	[0.07]
TREAT × *MF_SURP_GNEWS*	?	-1.626	0.203	6.296	2.233	8.163 **
		[-0.44]	[0.05]	[1.42]	[0.81]	[2.23]
TREAT × *MF_SURP_BNEWS*	?	-0.097	-0.148	-0.556	-0.722	-0.126
		[-0.07]	[-0.10]	[-0.53]	[-0.34]	[-0.04]
POST × *MF_SURP_GNEWS*	+	-0.130	3.825	8.811 *	6.057 **	4.954
		[-0.03]	[0.70]	[1.61]	[1.79]	[1.13]
POST × *MF_SURP_BNEWS*	+	0.733	0.695	0.740	1.946 ***	1.325 **
		[0.77]	[0.66]	[0.76]	[4.55]	[1.87]
TREAT × *POST* × *MF_SURP_GNEWS*	-	6.436	3.574	-4.142	-4.824	-8.445 **
		[1.08]	[0.60]	[-0.79]	[-1.21]	[-1.88]
TREAT × *POST* × *MF_SURP_BNEWS*	-	0.654	0.653	-0.302	1.116	1.336
		[0.42]	[0.42]	[-0.21]	[0.35]	[0.41]
HiOCFVOL	?		-0.009	-0.009		-0.003
			[-1.39]	[-1.42]		[-0.24]
HiOCFVOL × *MF_SURP_GNEWS*	-		-4.881 **	-4.014 **		-2.044
			[-1.78]	[-1.66]		[-0.64]
HiOCFVOL × *MF_SURP_BNEWS*	-		-0.144	-0.204		2.708
			[-0.15]	[-0.20]		[1.04]
MF_SURP_GNEWS × |*MF_SURP*|	-			33.807		
				[0.46]		
MF_SURP_BNEWS × |*MF_SURP*|	-			-60.452 ***		
				[-3.06]		
MF_LOSS	?			0.080 ***		
				[3.81]		
MF_LOSS × *MF_SURP_GNEWS*	-			-13.760 ***		
				[-4.16]		
MF_LOSS × *MF_SURP_BNEWS*	-			2.494 *		
				[1.65]		
HiMVE	?			0.007		0.004
				[0.92]		[0.30]
HiMVE × *MF_SURP_GNEWS*	-			-7.944 ***		-5.351 ***
				[-2.45]		[-3.75]
HiMVE × *MF_SURP_BNEWS*	-			-0.306		0.399
				[-0.23]		[0.15]

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TABLE 4.3 - *Continued*

		DV: $MF\_CAR_{0,1}$				
	Predicted	Unmatched Sample			Matched Sample	
Variable	Sign	(1)	(2)	(3)	(4)	(5)
EA_CONCUR	?					-0.004
						[-0.32]
EA_CONCUR × MF_SURP_GNEWS	?					4.420 *
						[1.95]
EA_CONCUR × MF_SURP_BNEWS	?					-1.444
						[-1.41]
Constant	?	-0.022 **	-0.015	-0.013	-0.017	-0.016
		[-2.29]	[-1.53]	[-1.25]	[-1.46]	[-1.07]
N		828	828	828	318	318
Adjusted R^2		0.139	0.156	0.194	0.157	0.167
df_m		11	14	22	11	20
df_r		283	283	283	111	111

This table presents the tests of H1, which hypothesizes that greater exposure to fair value accounting reduces the credibility of voluntary forward-looking disclosures. Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. Columns (1) - (3) report the results for the unmatched sample and columns (4) and (5) report the results for the matched sample. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.4

Constant Derivative Samples – H1

Panel A: Constant Derivative Unmatched Sample

	Number of Firm-Years			Number of Firms		
	Control	Treat	Total	Control	Treat	Total
Unmatched sample (table 3.1, panel B)	263	565	828	97	187	284
Less: Treatment firms that stop using derivatives between pre- to post-period	-	(29)	(29)	-	(9)	(9)
Less: Control firms that begin using derivatives between pre- to post-period	(125)	-	(125)	(44)	-	(44)
Set of observations with at least one post-period observation whose derivative use (non-use) is consistent with classification in the latest pre-period	138	536	674	53	178	231
Less: firm-years whose derivative use (non-use) is inconsistent with classification in latest pre-period	(3)	(4)	(7)	-	-	-
Constant derivative unmatched sample	135	532	667	53	178	231

Panel B: Constant Derivative Matched Sample

	Number of Firm-Years			Number of Firms		
	Control	Treat	Total	Control	Treat	Total
Matched sample (table 3.1, panel B)	153	165	318	56	56	112
Less: Treatment firms that stop using derivatives between pre- to post-period	-	(14)	(14)	-	(4)	(4)
Less: Control firms that begin using derivatives between pre- to post-period	(82)	-	(82)	(29)	-	(29)
Set of observations with at least one post-period observation whose derivative use (non-use) consistent with classification in the latest pre-period	71	151	222	27	52	79
Less: firm-years whose derivative use (non-use) is inconsistent with classification in latest pre-period	(1)	(2)	(3)	-	-	-
Constant derivative matched sample	70	149	219	27	52	79

Panels A and B of this table report the number of firms and their respective firm-years that enter the constant derivative unmatched and matched samples, respectively. The constant derivative samples include only those firms in the unmatched and matched sample, identified in panel B of table 3.1, with at least one post-period observation whose derivative use (non-use) is consistent with that in the latest pre-period and excludes any firm-years whose derivative use (non-use) is inconsistent with that in the latest pre-period.

Table 4.5

Covariate Balance in Constant Derivative Matched Sample - H1

Panel A: Combined Constant Derivative Matched Sample

	Control (N = 27)		Treatment (N = 52)		Difference in Means	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	0.059	0.044	0.046	0.032	-0.012	-1.28
MVE	6.684	1.550	7.343	1.734	0.659	1.72

Panel B: Good News Constant Derivative Matched Subsample

	Control (N = 12)		Treatment (N = 23)		Difference in Means	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	0.067	0.049	0.053	0.035	-0.015	-0.93
MVE	6.630	1.465	7.227	1.620	0.597	1.1

Panel C: Bad News Constant Derivative Matched Subsample

	Control (N = 15)		Treatment (N = 29)		Difference in Means	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	0.052	0.040	0.041	0.029	-0.010	-0.87
MVE	6.726	1.665	7.434	1.843	0.707	1.29

This table reports the difference in the covariate means between the treatment and control groups in the constant derivative matched H1 sample (table 4.4, panel B) in the latest pre-period year, the year of the match. Panels A, B and C report the descriptive statistics and the t-test results for the combined, the good news (positive *MF_SURP*), and the bad news (negative *MF_SURP*) constant derivative matched sample/subsamples, respectively. The matched sample is identified using CEM on *OCFVOL* (5 cutpoints), *MVE* (4 cutpoints), the Fama-French 12 industry classifications and fiscal year (table 3.1, panel B). All variables are defined in Appendix A.

TABLE 4.6

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Constant Derivative Sample (Test of H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$			
	Predicted	Constant Derivative Unmatched Sample		Constant Derivative Matched Sample	
Variable	Sign	(1)	(2)	(3)	(4)
TREAT	?	0.002	0.006	-0.022	-0.018
		[0.15]	[0.40]	[-0.97]	[-0.77]
POST	?	0.036 **	0.043 **	0.042 *	0.045 *
		[2.07]	[2.45]	[1.73]	[1.93]
TREAT × POST	?	-0.008	-0.013	0.008	0.003
		[-0.43]	[-0.67]	[0.28]	[0.10]
MF_SURP	+	3.404 ***	5.239 ***	1.754 ***	3.725 ***
		[2.61]	[2.68]	[4.50]	[2.45]
TREAT × MF_SURP	?	-1.911	-0.881	0.119	-0.499
		[-1.31]	[-0.63]	[0.09]	[-0.39]
POST × MF_SURP	+	-0.249	2.062	2.513 ***	3.272 ***
		[-0.16]	[1.24]	[4.51]	[3.58]
TREAT × POST × MF_SURP	-	3.342 *	-0.323	-0.692	-1.597
		[1.64]	[-0.15]	[-0.38]	[-0.74]
HiOCFVOL	?		-0.017 **		
			[-2.33]		
HiOCFVOL × MF_SURP	-		-2.701 **		
			[-1.73]		
MF_SURP × |MF_SURP|	-		-34.634		-35.027
			[-1.18]		[-1.52]
MF_LOSS	?		0.003		
			[0.10]		
MF_LOSS × MF_SURP	-		-1.323		
			[-0.79]		
HiMF_WIDTH	?		-0.013 *		
			[-1.62]		
HiMF_WIDTH × MF_SURP	-		2.048 *		
			[1.93]		
HiMVE	?		-0.011 *		
			[-1.42]		
HiMVE × MF_SURP	-		-1.317		
			[-1.15]		
Constant		-0.029 **	-0.013	-0.029	-0.030
		[-2.24]	[-0.89]	[-1.53]	[-1.59]
N		667	667	219	219
Adjusted R^2		0.141	0.181	0.159	0.163
df_m		7	16	7	8
df_r		230	230	78	78

Continued on next page

TABLE 4.6 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$							
		Constant Derivative Unmatched Sample				Constant Derivative Matched Sample			
Variable	Predicted Sign	(1)		(2)		(3)		(4)	
TREAT	?	0.000		-0.005		-0.029		-0.037	
		[-0.01]		[-0.34]		[-1.07]		[-1.27]	
POST	?	0.037	**	0.026		0.029		0.027	
		[2.08]		[1.39]		[1.07]		[1.00]	
TREAT × *POST*	?	-0.018		-0.009		0.026		0.035	
		[-0.93]		[-0.46]		[0.79]		[1.01]	
MF_SURP_GNEWS	+	1.439		0.186		0.300		0.162	
		[0.59]		[0.06]		[0.17]		[0.08]	
MF_SURP_BNEWS	+	3.660	**	6.629	***	2.014	***	1.238	***
		[2.44]		[2.76]		[5.23]		[0.26]	
TREAT × *MF_SURP_GNEWS*	?	-0.087		7.909	**	2.102		9.325	***
		[-0.03]		[2.10]		[0.75]		[2.69]	
TREAT × *MF_SURP_BNEWS*	?	-2.107		-2.440	*	-0.904		-1.774	
		[-1.20]		[-1.69]		[-0.41]		[-0.43]	
POST × *MF_SURP_GNEWS*	+	-0.429		20.287	***	12.239	*	13.541	*
		[-0.09]		[2.88]		[1.60]		[1.51]	
POST × *MF_SURP_BNEWS*	+	-0.334		1.009		1.990	***	0.020	
		[-0.21]		[0.57]		[5.07]		[0.01]	
TREAT × *POST* × *MF_SURP_GNEWS*	-	6.514		-15.529	**	-11.354	*	-16.759	**
		[1.14]		[-2.33]		[-1.43]		[-1.96]	
TREAT × *POST* × *MF_SURP_BNEWS*	-	1.745		-0.725		1.136		3.214	
		[0.88]		[-0.33]		[0.35]		[0.81]	
HiOCFVOL	?			-0.011				-0.006	
				[-1.44]				[-0.40]	
HiOCFVOL × *MF_SURP_GNEWS*	-			-4.721	**			-3.366	
				[-1.89]				[-1.01]	
HiOCFVOL × *MF_SURP_BNEWS*	-			-0.614				2.923	
				[-0.37]				[0.93]	
MF_SURP_GNEWS × |*MF_SURP*|	-			65.425					
				[0.83]					
MF_SURP_BNEWS × |*MF_SURP*|	-			-45.492	*				
				[-1.56]					
MF_LOSS	?			0.093	***				
				[3.15]					
MF_LOSS × *MF_SURP_GNEWS*	-			-23.271	***				
				[-4.11]					
MF_LOSS × *MF_SURP_BNEWS*	-			0.397					
				[0.22]					

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TABLE 4.6 - *Continued*

		DV: $MF\_CAR_{0,1}$			
		Constant Derivative Unmatched Sample		Constant Derivative Matched Sample	
Variable	Predicted Sign	(1)	(2)	(3)	(4)
HiMVE	?		0.008		0.005
			[1.07]		[0.35]
HiMVE × MF_SURP_GNEWS	-		-8.875 ***		-5.449 ***
			[-2.69]		[-3.41]
HiMVE × MF_SURP_BNEWS	-		0.671		1.657
			[0.46]		[0.46]
EA_CONCUR	?				-0.009
					[-0.65]
EA_CONCUR × MF_SURP_GNEWS	?				4.208
					[1.63]
EA_CONCUR × MF_SURP_BNEWS	?				-2.301
					[-1.20]
Constant	?	-0.026 *	-0.018	-0.026	-0.021
		[-1.84]	[-1.16]	[-1.23]	[-0.85]
N		667	667	219	219
Adjusted R^2		0.149	0.22	0.148	0.161
df_m		11	22	11	20
df_r		230	230	78	78

This table presents the tests of H1, which hypothesizes that greater exposure to fair value accounting reduces the credibility of voluntary forward-looking disclosures, using the constant derivative samples (table 4.4). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. Columns (1) and (2) report the results for the constant derivative unmatched sample and columns (3) and (4) report the results for the constant derivative matched sample. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.7

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Include Firm Fixed Effects (Test of H1)

Panel A: Management forecast response coefficient, not conditioned on sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$	
		Constant Derivative	Constant Derivative
	Predicted	Unmatched Sample	Matched Sample
Variable	Sign	(1)	(2)
POST	?	0.056 ***	0.055 **
		[3.15]	[2.43]
TREAT × POST	?	-0.021	-0.010
		[-1.08]	[-0.38]
MF_SURP	+	3.478	-1.761
		[1.08]	[-1.09]
TREAT × MF_SURP	?	2.439	5.582 **
		[1.08]	[2.62]
POST × MF_SURP	+	5.282 **	7.292 ***
		[2.28]	[9.03]
TREAT × POST × MF_SURP	-	-2.889	-6.514 ***
		[-0.98]	[-3.00]
HiOCFVOL	?	-0.020 *	
		[-1.82]	
HiOCFVOL × MF_SURP	-	-3.004 **	
		[-1.82]	
MF_SURP × |MF_SURP|	-	-32.689	-13.443
		[-0.87]	[-0.54]
MF_LOSS	?	-0.036	
		[-0.44]	
MF_LOSS × MF_SURP	-	-2.261	
		[-0.88]	
HiMF_WIDTH	?	-0.023 *	
		[-1.87]	
HiMF_WIDTH × MF_SURP	-	1.131	
		[0.77]	
HiMVE	?	-0.046 **	
		[-2.04]	
HiMVE × MF_SURP	-	-1.977	
		[-1.13]	
Constant	?	0.014	-0.043 ***
		[1.03]	[-7.06]
Firm fixed effects		Yes	Yes
N		667	219
Adjusted R^2		0.236	0.202
df_m		14	6
df_r		230	78

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TABLE 4.7 - *Continued*

Panel B: Management forecast response coefficient, conditioned on sign of *MF_SURP*			
		DV: $MF\_CAR_{0,1}$	
	Predicted	Constant Derivative Unmatched Sample	Constant Derivative Matched Sample
Variable	Sign	(1)	(2)
POST	?	0.055 **	0.052 *
		[2.47]	[1.93]
TREAT × POST	?	-0.028	0.016
		[-1.18]	[0.42]
MF_SURP_GNEWS	+	0.632	-2.145
		[0.13]	[-0.56]
MF_SURP_BNEWS	+	3.657	-6.344
		[1.07]	[-1.23]
TREAT × MF_SURP_GNEWS	?	14.731 ***	17.390 ***
		[2.83]	[3.86]
TREAT × MF_SURP_BNEWS	?	0.960	2.478
		[0.44]	[0.51]
POST × MF_SURP_GNEWS	+	15.257 **	8.109
		[1.92]	[0.91]
POST × MF_SURP_BNEWS	+	5.552 **	4.362 **
		[2.11]	[1.95]
TREAT × POST × MF_SURP_GNEWS	-	-13.897 **	-13.804 *
		[-1.83]	[-1.66]
TREAT × POST × MF_SURP_BNEWS	-	-3.882	-1.278
		[-1.28]	[-0.24]
HiOCFVOL	?	-0.005	0.006
		[-0.44]	[0.27]
HiOCFVOL × MF_SURP_GNEWS	-	-8.629 ***	-11.637 ***
		[-2.75]	[-3.51]
HiOCFVOL × MF_SURP_BNEWS	-	0.098	6.953 *
		[0.04]	[1.59]
MF_SURP_GNEWS × |MF_SURP|	-	-16.915	
		[-0.19]	
MF_SURP_BNEWS × |MF_SURP|	-	-61.638 **	
		[-1.78]	
MF_LOSS	?	0.079	
		[1.16]	
MF_LOSS × MF_SURP_GNEWS	-	-15.729 ***	
		[-2.60]	
MF_LOSS × MF_SURP_BNEWS	-	-0.137	
		[-0.05]	

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TABLE 4.7 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)	Constant Derivative Matched Sample (2)
HiMVE	?	-0.016	-0.059
		[-0.72]	[-1.32]
HiMVE × MF_SURP_GNEWS	-	-12.129 ***	2.075
		[-3.15]	[0.21]
HiMVE × MF_SURP_BNEWS	-	0.691	3.859
		[0.37]	[0.94]
EA_CONCUR	?		-0.008
			[-0.41]
EA_CONCUR × MF_SURP_GNEWS	?		7.579 *
			[1.68]
EA_CONCUR × MF_SURP_BNEWS	?		-3.455
			[-1.54]
Constant	?	-0.016	-0.039
		[-1.13]	[-1.37]
Firm Fixed Effects		Yes	Yes
N		667	219
Adjusted R^2		0.277	0.257
df_m		20	18
df_r		230	78

This table reports the results of the additional analysis of H1 after including firm fixed effects, using the constant derivative samples (table 4.4). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. Column (1) and (2) report the results for the constant derivative unmatched sample and matched sample, respectively. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.8

Descriptive Statistics for Control versus Treatment Firms - Alternative Matched Sample (H1)

Panel A: Pre-Treatment Covariate Balance in Combined Alternative Matched Sample

	Control			Treatment			Difference in Means	
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	28	0.056	0.043	28	0.040	0.028	-0.016	-1.67
MVE	28	6.633	1.536	28	7.251	1.547	0.617	1.50

Panel B: MF and Firm-Level Characteristics in Combined Alternative Matched Subsample

	Control			Treatment			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
$MF\_CAR_{0,1}$	71	-0.019	0.108	81	-0.022	0.108	-0.002	-0.14
MF_SURP	71	-0.004	0.015	81	-0.001	0.008	0.003	1.59
MF_BNEWS	71	0.535	0.502	81	0.444	0.500	-0.091	-1.11
OCFVOL	71	0.049	0.033	81	0.039	0.025	-0.010	-2.03 *
MF_SURP × |*MF_SURP*|	71	0.000	0.001	81	0.0000	0.0002	0.0002	1.55
MF_LOSS	71	0.014	0.119	81	0.000	0.000	-0.014	-1.00
MF_WIDTH	71	0.002	0.005	81	0.002	0.004	0.000	-0.22
MF_HORIZON	71	4.832	0.732	81	4.563	0.820	-0.269	-2.14 *
MTB	71	3.409	3.382	81	6.935	26.879	3.527	1.17
MVE	71	6.735	1.620	81	7.357	1.663	0.622	2.33 *
EA_CONCUR	71	0.577	0.497	81	0.531	0.502	-0.047	-0.57
EA_SURP	41	0.001	0.044	43	-0.005	0.045	-0.005	-0.55
EA_SURP × |*EA_SURP*|	41	0.001	0.008	43	-0.002	0.012	-0.002	-1.06
EA_LOSS	41	0.098	0.300	43	0.093	0.294	-0.005	-0.07

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TABLE 4.8 – *Continued*

Panel C: MF and Firm-Level Characteristics in Good News Alternative Matched Subsample

	Control			Treatment			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
$MF\_CAR_{0,1}$	33	0.015	0.083	45	0.009	0.077	-0.006	-0.32
MF_SURP	33	0.002	0.004	45	0.003	0.006	0.000	0.31
OCFVOL	33	0.050	0.034	45	0.045	0.030	-0.005	-0.65
$MF\_SURP \times \lvert MF\_SURP\rvert$	33	0.000	0.000	45	0.000	0.000	0.000	0.69
MF_LOSS	33	0.000	0.000	45	0.000	0.000	0.000	0.00
MF_WIDTH	33	0.002	0.005	45	0.002	0.005	0.000	-0.03
MF_HORIZON	33	5.041	0.600	45	4.545	0.914	-0.496	-2.89 **
MTB	33	3.887	4.519	45	4.718	10.925	0.831	0.46
MVE	33	6.944	1.598	45	7.196	1.714	0.251	0.67
EA_CONCUR	33	0.667	0.479	45	0.533	0.505	-0.133	-1.19
EA_SURP	22	0.005	0.058	24	-0.008	0.060	-0.012	-0.71
$EA\_SURP \times \lvert EA\_SURP\rvert$	22	0.001	0.012	24	-0.0031	0.016	-0.004	-1.08
EA_LOSS	22	0.045	0.213	24	0.125	0.338	0.080	0.96

Panel D: MF and Firm-Level Characteristics in Bad News Alternative Matched Subsample

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
$MF\_CAR_{0,1}$	38	-0.049	0.118	36	-0.060	0.129	-0.011	-0.38
MF_SURP	38	-0.009	0.019	36	-0.005	0.007	0.004	1.32
OCFVOL	38	0.049	0.033	36	0.032	0.016	-0.016	-2.74 **
$MF\_SURP \times \lvert MF\_SURP\rvert$	38	0.000	0.002	36	0.000	0.000	0.000	1.42
MF_LOSS	38	0.026	0.162	36	0.000	0.000	-0.026	-1.00
MF_WIDTH	38	0.003	0.005	36	0.003	0.004	0.000	-0.11
MF_HORIZON	38	4.650	0.793	36	4.585	0.698	-0.065	-0.38
MTB	38	2.993	1.895	36	9.707	38.563	6.714	1.04
MVE	38	6.553	1.638	36	7.559	1.599	1.006	2.67 **
EA_CONCUR	38	0.500	0.507	36	0.528	0.506	0.028	0.24
EA_SURP	19	-0.004	0.018	19	-0.001	0.014	0.003	0.56
$EA\_SURP \times \lvert EA\_SURP\rvert$	19	0.000	0.001	19	0.000	0.000	0.000	0.61
EA_LOSS	19	0.158	0.375	19	0.053	0.229	-0.105	-1.04

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Panel A of this table reports the descriptive statistics of covariates used to identify the alternative matched sample and the results of the t-tests of covariate means between control and treatment groups in the latest pre-period, the year of the match. Panels B, C and D report the descriptive statsitics of management forecast and firm characteristics, by control and treatment firms, as well as the results of the t-test of means for the combined, the good news (positive *MF_SURP*), and the bad news (negative *MF_SURP*) alternative matched sample/subsamples, respectively. The alternative matched sample is identified using CEM on *OCFVOL* (5 cutpoints), *MVE* (4 cutpoints), the Fama-French 12 industry classifications and fiscal year within the constant derivative unmatched sample (table 4.4, panel A). For ease of interpretation, the summary statistics for *EA_SURP, EA_SURP* × |*EA_SURP*| and *EA_LOSS* are based only on management forecasts that are issued concurrently with an earnings announcement. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the two-tailed tests. All variables are defined in Appendix A.

TABLE 4.9

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Alternative Matched Sample (Test of H1)

Panel A: Management forecast response coefficient, not conditioned on sign of *MF_SURP*

Variable	Predicted sign	DV: $MF\_CAR_{0,1}$ (1)		(2)		(3)	
TREAT	?	-0.011		-0.008		-0.009	
		[-0.46]		[-0.34]		[-0.37]	
POST	?	0.036		0.039		0.038	
		[1.48]		[1.62]		[1.56]	
TREAT × *POST*	?	-0.003		-0.006		-0.005	
		[-0.11]		[-0.19]		[-0.15]	
MF_SURP	+	0.460		1.576	***	1.697	***
		[0.24]		[3.13]		[3.53]	
TREAT × *MF_SURP*	?	5.306	*	4.372		4.322	
		[1.79]		[1.53]		[1.54]	
POST × *MF_SURP*	+	2.180	***	2.386	***	2.366	***
		[4.30]		[3.69]		[3.96]	
TREAT × *POST* × *MF_SURP*	-	-2.646		-2.071		-1.842	
		[-0.77]		[-0.62]		[-0.60]	
HiOCFVOL	?	-0.006					
		[-0.40]					
HiOCFVOL × *MF_SURP*	-	1.350					
		[0.66]					
HiMF_HORIZON	?			0.005			
				[0.32]			
HiMF_HORIZON × *MF_SURP*	-			0.290			
				[0.26]			
HiMVE	?					-0.007	
						[-0.44]	
HiMVE × *MF_SURP*	-					-0.437	
						[-0.08]	
Constant	?	-0.021		-0.030		-0.024	
		[-1.09]		[-1.48]		[-1.28]	
N		152		152		152	
Adjusted R^2		0.199		0.197		0.198	
df_m		9		9		9	
df_r		55		55		55	

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TABLE 4.9 - *Continued*

Panel B: Management forecast response coefficient, conditioned on sign of *MF_SURP*

Variable	Predicted Sign	DV: *MF_CAR* $_{0,1}$ (1)		(2)		(3)	
TREAT	?	-0.033		-0.030		-0.031	
		[-1.07]		[-0.96]		[-0.96]	
POST	?	0.034		0.033		0.031	
		[1.22]		[1.15]		[1.09]	
TREAT × *POST*	?	0.026		0.021		0.021	
		[0.67]		[0.52]		[0.53]	
MF_SURP_GNEWS	+	-0.092		-0.112		0.618	
		[-0.05]		[-0.03]		[0.28]	
MF_SURP_BNEWS	+	2.322		1.754	***	1.870	***
		[0.57]		[3.85]		[4.00]	
TREAT × *MF_SURP_GNEWS*	?	10.701	***	11.055	**	10.282	***
		[3.78]		[2.34]		[3.42]	
TREAT × *MF_SURP_BNEWS*	?	-3.003		-2.877		-3.128	
		[-0.71]		[-0.97]		[-0.93]	
POST × *MF_SURP_GNEWS*	+	-1.617		8.517		8.006	
		[-0.24]		[1.10]		[1.13]	
POST × *MF_SURP_BNEWS*	+	2.104	***	1.995	***	2.014	***
		[4.51]		[3.73]		[4.48]	
TREAT × *POST* × *MF_SURP_GNEWS*	-	-13.000	**	-13.113	*	-11.785	*
		[-1.76]		[-1.31]		[-1.55]	
TREAT × *POST* × *MF_SURP_BNEWS*	-	5.346		4.883		4.959	*
		[1.21]		[1.27]		[1.35]	
HiOCFVOL	?	-0.017					
		[-0.92]					
HiOCFVOL × *MF_SURP_GNEWS*	-	12.263	***				
		[3.95]					
HiOCFVOL × *MF_SURP_BNEWS*	-	-0.665					
		[-0.16]					
HiMF_HORIZON	?			0.010			
				[0.51]			
HiMF_HORIZON × *MF_SURP_GNEWS*	-			0.583			
				[0.17]			
HiMF_HORIZON × *MF_SURP_BNEWS*	-			0.599			
				[0.46]			

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TABLE 4.9 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ (1)	(2)	(3)
HiMVE	?			0.002
				[0.09]
HiMVE × *MF_SURP_GNEWS*	-			-3.718
				[-0.27]
HiMVE × *MF_SURP_BNEWS*	-			2.175
				[0.37]
Constant	?		-0.030	-0.024
			[-1.28]	[-1.09]
N		152	152	152
Adjusted R^2		0.206	0.194	0.193
df_m		14	14	14
df_r		55	55	55

This table reports the results of the additional analysis of H1, using the alternative matched sample. The alternative matched sample is identified using CEM on *OCFVOL* (5 cutpoints), *MVE* (4 cutpoints), the Fama-French 12 industry classifications and fiscal year within the constant derivative unmatched sample (table 4.4, panel A). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.10

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Stand-Alone Management Forecasts (Test of H1)

Panel A: Management forecast response coefficient, not conditioned on sign of *MF_SURP*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample
TREAT	?	0.016
		[0.94]
POST	?	0.049 *
		[1.82]
TREAT × POST	?	-0.023
		[-0.82]
MF_SURP	+	8.114 ***
		[3.17]
TREAT × MF_SURP	?	-1.481
		[-0.68]
POST × MF_SURP	+	2.574
		[1.01]
TREAT × POST × MF_SURP	-	-1.497
		[-0.51]
HiOCFVOL	?	-0.006
		[-0.70]
HiOCFVOL × MF_SURP	-	-2.866 ***
		[-2.61]
MF_SURP × |MF_SURP|	-	-72.563 ***
		[-3.47]
MF_LOSS	?	0.047 *
		[1.85]
MF_LOSS × MF_SURP	-	0.826
		[0.75]
HiMF_WIDTH	?	-0.039 ***
		[-3.18]
HiMF_WIDTH × MF_SURP	-	1.365 *
		[1.35]
HiMVE	?	-0.007
		[-0.75]
HiMVE × MF_SURP	-	-1.069
		[-1.20]
Constant	?	-0.02
		[-1.23]
N		312
Adjusted R^2		0.249
df_m		16
df_r		190

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TABLE 4.10 – *Continued*

Panel B: Management forecast response coefficient, conditioned on sign of *MF_SURP*

Variable	Predicted Sign	DV: *MF_CAR* $_{0,1}$ Constant Derivative Unmatched Sample
TREAT	?	0.004
		[0.20]
POST	?	0.006
		[0.18]
TREAT × *POST*	?	0.018
		[0.53]
MF_SURP_GNEWS	+	0.253
		[0.16]
MF_SURP_BNEWS	+	14.933 ***
		[7.39]
TREAT × *MF_SURP_GNEWS*	?	6.543 *
		[1.94]
TREAT × *MF_SURP_BNEWS*	?	-4.056 ***
		[-3.07]
POST × *MF_SURP_GNEWS*	+	35.553 ***
		[3.94]
POST × *MF_SURP_BNEWS*	+	-1.765
		[-0.99]
TREAT × *POST* × *MF_SURP_GNEWS*	-	-41.710 ***
		[-4.08]
TREAT × *POST* × *MF_SURP_BNEWS*	-	3.387
		[1.24]
HiOCFVOL	?	-0.014
		[-1.38]
HiOCFVOL × *MF_SURP_GNEWS*	-	-1.189
		[-1.12]
HiOCFVOL × *MF_SURP_BNEWS*	-	-4.094 ***
		[-3.06]
MF_SURP_GNEWS × |*MF_SURP*|	-	-33.277
		[-0.97]
MF_SURP_BNEWS × |*MF_SURP*|	-	-103.484 ***
		[-3.71]
MF_LOSS	?	0.076 ***
		[2.92]
MF_LOSS × *MF_SURP_GNEWS*	-	-10.893 **
		[-1.91]
MF_LOSS × *MF_SURP_BNEWS*	-	4.052 ***
		[2.37]

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TABLE 4.10 - *Continued*

		DV: $MF\_CAR_{0,1}$
Variable	Predicted Sign	Constant Derivative Unmatched Sample
HiMVE	?	-0.008
		[-0.65]
HiMVE × MF_SURP_GNEWS	-	-3.725
		[-1.14]
HiMVE × MF_SURP_BNEWS	-	-3.161 **
		[-1.98]
Constant	?	-0.013
		[-0.69]
N		312
Adjusted R^2		0.228
df_m		22
df_r		190

This table reports the results of the additional analysis of H1, using only stand-alone forecasts in the constant derivative unmatched sample (table 4.4, panel A). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.11

Descriptive Statistics for Control Group versus Treatment Group - Earliest MF (H1)

Panel A.1: Combined Constant Derivative Unmatched Sample

	Control			Treat			Difference in Means		
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat	
MF_SURP	135	-0.003	0.015	532	-0.001	0.011	0.002	1.44	
MF_BNEWS	135	0.452	0.500	532	0.500	0.500	0.048	1.00	
OCFVOL	135	0.077	0.095	532	0.039	0.030	-0.038	-4.61	***
MF_SURP × |*MF_SURP*|	135	0.000	0.001	532	0.0000	0.0006	0.0002	1.80	
MF_LOSS	135	0.059	0.237	532	0.004	0.061	-0.055	-2.70	**
MF_WIDTH	135	0.003	0.007	532	0.002	0.004	-0.001	-1.07	
MF_HORIZON	135	5.259	0.665	532	5.333	0.680	0.073	1.14	
MTB	135	3.792	3.469	532	4.707	19.463	0.915	1.02	
SIZE	135	6.373	1.515	532	8.152	1.746	1.779	11.80	***
EA_CONCUR	135	0.489	0.502	532	0.650	0.477	0.161	3.37	***
EA_SURP	66	-0.014	0.121	346	-0.005	0.073	0.009	0.56	
EA_SURP × |*EA_SURP*|	66	-0.011	0.080	346	-0.004	0.070	0.007	0.67	
EA_LOSS	66	0.182	0.389	346	0.171	0.377	-0.011	-0.22	

Panel A.2: Good News Constant Derivative Unmatched Subsample

	Control			Treat			Difference in Means		
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat	
MF_SURP	74	0.003	0.005	266	0.004	0.009	0.001	0.95	
OCFVOL	74	0.073	0.093	266	0.040	0.029	-0.033	-3.04	**
MF_SURP × |*MF_SURP*|	74	0.000	0.000	266	0.000	0.001	0.000	1.36	
MF_LOSS	74	0.041	0.199	266	0.000	0.000	-0.041	-1.76	
MF_WIDTH	74	0.003	0.008	266	0.002	0.004	0.000	-0.31	
MF_HORIZON	74	5.442	0.400	266	5.396	0.601	-0.046	-0.78	
MTB	74	4.196	4.104	266	3.375	13.934	-0.821	-0.84	
MVE	74	6.532	1.627	266	8.029	1.740	1.497	6.89	***
EA_CONCUR	74	0.541	0.502	266	0.669	0.471	0.129	1.98	
EA_SURP	40	-0.033	0.144	178	-0.007	0.098	0.025	1.07	
EA_SURP × |*EA_SURP*|	40	-0.021	0.102	178	-0.008	0.097	0.013	0.74	
EA_LOSS	40	0.200	0.405	178	0.1854	0.3897	-0.015	-0.21	

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TABLE 4.11 - *Continued*

Panel A.3: Bad News Constant Derivative Unmatched Subsample

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
MF_SURP	61	-0.011	0.020	266	-0.006	0.011	0.005	1.74
OCFVOL	61	0.082	0.098	266	0.038	0.031	-0.044	-3.46 ***
MF_SURP × |*MF_SURP*|	61	-0.001	0.002	266	0.000	0.001	0.000	1.71
MF_LOSS	61	0.082	0.277	266	0.008	0.087	-0.074	-2.08 *
MF_WIDTH	61	0.003	0.005	266	0.002	0.003	-0.001	-1.59
MF_HORIZON	61	5.037	0.837	266	5.269	0.747	0.232	1.99 *
MTB	61	3.303	2.437	266	6.040	23.692	2.737	1.84
MVE	61	6.180	1.355	266	8.274	1.746	2.095	10.3 ***
EA_CONCUR	61	0.426	0.499	266	0.632	0.483	0.205	2.92 **
EA_SURP	26	0.016	0.065	168	-0.003	0.032	-0.018	-1.40
EA_SURP × |*EA_SURP*|	26	0.004	0.016	168	0.000	0.004	-0.004	-1.26
EA_LOSS	26	0.154	0.368	168	0.155	0.363	0.001	0.01

Panel B.1: Combined Constant Derivative Matched Sample

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
$MF\_CAR_{0,1}$	153	-0.007	0.092	*165*	-0.024	0.108	-0.018	-1.44
MF_SURP	70	-0.003	0.017	*149*	-0.001	0.010	0.002	1.04
MF_BNEWS	70	0.400	0.493	*149*	0.456	0.500	0.056	0.79
OCFVOL	70	0.050	0.035	*149*	0.044	0.032	-0.006	-1.16
MF_SURP × |*MF_SURP*|	70	-0.0003	0.0012	*149*	0.0000	0.0004	0.0002	1.63
MF_LOSS	70	0.014	0.120	*149*	0.000	0.000	-0.014	-1
MF_WIDTH	70	0.002	0.005	*149*	0.002	0.004	0.0001	0.12
MF_HORIZON	70	5.258	0.687	*149*	5.297	0.781	0.039	0.38
MTB	70	3.435	3.389	*149*	4.094	24.388	0.659	0.32
SIZE	70	6.733	1.630	*149*	7.508	1.807	1.781	3.17 **
EA_CONCUR	70	0.500	0.504	*149*	0.591	0.493	0.091	1.25
EA_SURP	35	-0.002	0.029	88	0.001	0.028	0.004	0.63
EA_SURP × |*EA_SURP*|	35	-0.001	0.004	88	0.000	0.003	0.001	1.10
EA_LOSS	35	0.143	0.355	88	0.148	0.357	0.005	0.07

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TABLE 4.11 – *Continued*

Panel B.2: Good News Constant Derivative Matched Subsample

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
MF_SURP	42	0.003	0.004	81	0.004	0.008	0.001	0.94
OCFVOL	42	0.048	0.034	81	0.047	0.035	-0.001	-0.19
MF_SURP × |*MF_SURP*|	42	0.000	0.000	81	0.000	0.000	0.000	1.06
MF_LOSS	42	0.000	0.000	81	0.000	0.000	0.000	0.00
MF_WIDTH	42	0.002	0.004	81	0.003	0.005	0.001	0.95
MF_HORIZON	42	5.427	0.438	81	5.378	0.672	-0.049	-0.49
MTB	42	3.714	4.148	81	1.061	18.397	-2.653	-1.24
MVE	42	6.806	1.618	81	7.373	1.825	0.567	1.76
EA_CONCUR	42	0.548	0.504	81	0.630	0.486	0.082	0.87
EA_SURP	23	-0.003	0.031	51	0.004	0.023	0.007	0.97
EA_SURP × |*EA_SURP*|	23	-0.001	0.004	51	0.000	0.002	0.001	1.33
EA_LOSS	23	0.130	0.344	51	0.157	0.367	0.026	0.30

Panel B.3: Bad News Constant Derivative Matched Subsample

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
MF_SURP	28	-0.012	0.024	68	-0.007	0.009	0.006	1.21
OCFVOL	28	0.053	0.035	68	0.041	0.027	-0.012	-1.54
MF_SURP × |*MF_SURP*|	28	-0.001	0.002	68	0.000	0.000	0.001	1.58
MF_LOSS	28	0.036	0.189	68	0.000	0.000	-0.036	-1.00
MF_WIDTH	28	0.003	0.006	68	0.002	0.003	-0.001	-0.76
MF_HORIZON	28	5.003	0.896	68	5.200	0.890	0.197	0.98
MTB	28	3.017	1.710	68	7.708	29.755	4.691	1.29
MVE	28	6.623	1.672	68	7.670	1.785	1.047	2.73 **
EA_CONCUR	28	0.429	0.504	68	0.544	0.502	0.116	1.02
EA_SURP	12	0.000	0.024	37	-0.002	0.034	-0.002	-0.20
EA_SURP × |*EA_SURP*|	12	0.000	0.001	37	0.000	0.004	0.000	-0.19
EA_LOSS	12	0.167	0.389	37	0.135	0.347	-0.032	-0.25

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TABLE 4.11 - *Continued*

This table reports the descriptive statistics of management forecast and firm characteristics, by control and treatment firms, as well as the results of the t-test of means using the earliest management forecast for a given firm-year in the constant derivative samples (table 4.4). Panels A.1, A.2 and A.3 (B.1, B.2 and B.3) provide the descriptive statistics and t-test results for the combined, good news and bad news constant derivative unmatched (matched) sample/subsamples, respectively. For ease of interpretation, the summary statistics for *EA_SURP, EA_SURP* × |*EA_SURP*| and *EA_LOSS* are based only on management forecasts that are issued concurrently with an earnings announcement. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the two-tailed tests. All variables are defined in Appendix A.

TABLE 4.12

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Earliest Management Forecast in a Given Firm-Year (Test of H1)

Panel A: Management forecast response coefficient, not conditioned on sign of *MF_SURP*

Variable	Predicted sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)		Constant Derivative Matched Sample (2)	
TREAT	?	0.005		-0.014	
		[0.34]		[-0.65]	
POST	?	0.034	**	0.043	**
		[2.08]		[2.45]	
TREAT × POST	?	-0.009		0.004	
		[-0.50]		[0.17]	
MF_SURP	+	4.998	***	1.576	***
		[2.70]		[3.67]	
TREAT × MF_SURP	?	-1.204		2.152	
		[-0.72]		[1.59]	
POST × MF_SURP	+	0.252		2.312	***
		[0.16]		[3.89]	
TREAT × POST × MF_SURP	-	0.113		-0.420	
		[0.06]		[-0.19]	
HiOCFVOL	?	0.002			
		[0.38]			
HiOCFVOL × MF_SURP	-	-0.652			
		[-0.76]			
MF_LOSS	?	0.056	***		
		[2.79]			
MF_LOSS × MF_SURP	-	-0.823			
		[-0.97]			
HiMVE	?	0.003		0.005	
		[0.41]		[0.43]	
HiMVE × MF_SURP	-	-1.574	**	-2.717	**
		[-1.77]		[-1.73]	
EA_CONCUR	?	0.015	**		
		[2.16]			
EA_CONCUR × MF_SURP	?	-1.445	**		
		[-2.02]			
Constant	?	-0.037	***	-0.024	
		[-2.65]		[-1.54]	
N		667		219	
Adjusted R^2		0.159		0.237	
df_m		15		9	
df_r		230		78	

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TABLE 4.12 - *Continued*

Panel B: Management forecast response coefficient, conditioned on sign of *MF_SURP*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)	DV: $MF\_CAR_{0,1}$ Constant Derivative Matched Sample (2)
TREAT	?	0.002	-0.015
		[0.13]	[-0.60]
POST	?	0.013	0.030
		[0.76]	[1.56]
TREAT × POST	?	0.011	0.009
		[0.57]	[0.34]
MF_SURP_GNEWS	+	1.443	-0.458
		[0.65]	[-0.41]
MF_SURP_BNEWS	+	4.525 **	1.911 ***
		[1.74]	[6.27]
TREAT × MF_SURP_GNEWS	?	1.680	3.794
		[0.62]	[1.64]
TREAT × MF_SURP_BNEWS	?	-1.594	1.981
		[-0.72]	[1.06]
POST × MF_SURP_GNEWS	+	11.544 ***	8.446 ***
		[2.54]	[2.59]
POST × MF_SURP_BNEWS	+	-0.933	1.704 ***
		[-0.42]	[3.59]
TREAT × POST × MF_SURP_GNEWS	-	-10.970 ***	-4.762
		[-2.63]	[-1.28]
TREAT × POST × MF_SURP_BNEWS	-	0.961	-0.975
		[0.43]	[-0.30]
HiOCFVOL	?	0.005	
		[0.71]	
HiOCFVOL × MF_SURP_GNEWS	-	-1.298	
		[-1.07]	
HiOCFVOL × MF_SURP_BNEWS	-	1.212	
		[0.71]	
MF_LOSS	?	0.048 **	
		[2.24]	
MF_LOSS × MF_SURP_GNEWS	-	-6.057 **	
		[-1.78]	
MF_LOSS × MF_SURP_BNEWS	-	-1.039	
		[-0.96]	
HiMF_HORIZON	?	0.002	
		[0.39]	
HiMF_HORIZON × MF_SURP_GNEWS	-	-3.023 **	
		[-1.66]	
HiMF_HORIZON × MF_SURP_BNEWS	-	-0.432	
		[-0.47]	

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TABLE 4.12 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)	Constant Derivative Matched Sample (2)
HiMVE	?	0.010	0.010
		[1.29]	[0.69]
HiMVE × MF_SURP_GNEWS	-	-1.927	-2.904 *
		[-1.27]	[-1.44]
HiMVE × MF_SURP_BNEWS	-	1.062	-1.887
		[0.55]	[-0.56]
EA_CONCUR	?	0.005	
		[0.72]	
EA_CONCUR × MF_SURP_GNEWS	?	2.319	
		[1.16]	
EA_CONCUR × MF_SURP_BNEWS	?	-2.397 **	
		[-2.17]	
Constant	?	-0.032 **	-0.022
		[-2.20]	[-1.25]
N		667	219
Adjusted R^2		0.172	0.226
df_m		26	14
df_r		230	78

This table reports the results of the additional analysis of H1, using the earliest management forecast for a given firm-year in the constant derivative sample (table 4.4). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.13

Descriptive Statistics for Control Group versus Treatment Group - Constant Derivative Samples (H1)

Panel A.1: Combined Constant Derivative Unmatched Sample

	Control			Treat			Difference in Means		
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat	
MF_SURP	135	-0.003	0.015	532	-0.001	0.011	0.002	1.44	
MF_BNEWS	135	0.452	0.500	532	0.500	0.500	0.048	1.00	
OCFVOL	135	0.077	0.095	532	0.039	0.030	-0.038	-4.61	***
MF_SURP × |*MF_SURP*|	135	0.000	0.001	532	0.0000	0.0006	0.0002	1.80	
MF_LOSS	135	0.059	0.237	532	0.004	0.061	-0.055	-2.70	**
MF_WIDTH	135	0.003	0.007	532	0.002	0.004	-0.001	-1.07	
MF_HORIZON	135	5.259	0.665	532	5.333	0.680	0.073	1.14	
MTB	135	3.792	3.469	532	4.707	19.463	0.915	1.02	
MVE	135	6.373	1.515	532	8.152	1.746	1.779	11.80	***
EA_CONCUR	135	0.489	0.502	532	0.650	0.477	0.161	3.37	***
EA_SURP	66	-0.014	0.121	346	-0.005	0.073	0.009	0.56	
EA_SURP × |*EA_SURP*|	66	-0.011	0.080	346	-0.004	0.070	0.007	0.67	
EA_LOSS	66	0.182	0.389	346	0.171	0.377	-0.011	-0.22	

Panel A.2: Good News Constant Derivative Unmatched Subsample

	Control			Treat			Difference in Means		
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat	
MF_SURP	65	0.002	0.005	233	0.003	0.008	0.001	1.30	
OCFVOL	65	0.079	0.097	233	0.041	0.030	-0.038	-3.16	**
MF_SURP × |*MF_SURP*|	65	0.000	0.000	233	0.000	0.000	0.000	1.66	
MF_LOSS	65	0.046	0.211	233	0.000	0.000	-0.046	-1.76	
MF_WIDTH	65	0.003	0.006	233	0.002	0.003	-0.001	-0.94	
MF_HORIZON	65	4.985	0.657	233	4.583	0.952	-0.402	-3.92	***
MTB	65	3.885	3.922	233	4.178	14.795	0.293	0.27	
MVE	65	6.430	1.659	233	8.004	1.775	1.574	6.66	***
EA_CONCUR	65	0.585	0.497	233	0.549	0.499	-0.035	-0.51	
EA_SURP	38	0.012	0.056	128	0.000	0.035	-0.012	-1.29	
EA_SURP × |*EA_SURP*|	38	0.002	0.011	128	-0.001	0.007	-0.003	-1.42	
EA_LOSS	38	0.105	0.311	128	0.1016	0.3033	-0.004	-0.06	

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TABLE 4.13 - *Continued*

Panel A.3: Bad News Constant Derivative Unmatched Subsample

	Control			Treat			Difference in Means		
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat	
MF_SURP	70	-0.009	0.018	299	-0.006	0.011	0.003	1.37	
OCFVOL	70	0.075	0.095	299	0.038	0.030	-0.038	-3.31	**
MF_SURP × |*MF_SURP*|	70	0.000	0.001	299	0.000	0.001	0.000	1.54	
MF_LOSS	70	0.071	0.259	299	0.010	0.100	-0.061	-1.95	
MF_WIDTH	70	0.003	0.004	299	0.002	0.004	-0.001	-0.89	
MF_HORIZON	70	4.699	0.827	299	4.449	0.992	-0.250	-2.19	*
MTB	70	3.706	3.015	299	5.120	22.454	1.414	1.05	
MVE	70	6.320	1.378	299	8.267	1.717	1.947	10.12	***
EA_CONCUR	70	0.471	0.503	299	0.522	0.500	0.050	0.75	
EA_SURP	33	0.010	0.058	156	0.002	0.043	-0.009	-0.82	
EA_SURP × |*EA_SURP*|	33	0.003	0.014	156	0.001	0.011	-0.002	-0.72	
EA_LOSS	33	0.152	0.364	156	0.109	0.313	-0.043	-0.62	

Panel B.1: Combined Constant Derivative Matched Sample

	Control			Treat			Difference in Means		
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat	
MF_CAR $_{0,1}$	70	-0.007	0.092	149	-0.024	0.108	-0.018	-1.44	
MF_SURP	70	-0.003	0.017	149	-0.001	0.010	0.002	1.04	
MF_BNEWS	70	0.400	0.493	149	0.456	0.500	0.056	0.79	
OCFVOL	70	0.050	0.035	149	0.044	0.032	-0.006	-1.16	
MF_SURP × |*MF_SURP*|	70	-0.0003	0.0012	149	0.0000	0.0004	0.0002	1.63	
MF_LOSS	70	0.014	0.120	149	0.000	0.000	-0.014	-1	
MF_WIDTH	70	0.002	0.005	149	0.002	0.004	0.0001	0.12	
MF_HORIZON	70	5.258	0.687	149	5.297	0.781	0.039	0.38	
MTB	70	3.435	3.389	149	4.094	24.388	0.659	0.32	
MVE	70	6.733	1.630	149	7.508	1.807	1.781	3.17	**
EA_CONCUR	70	0.500	0.504	149	0.591	0.493	0.091	1.25	
EA_SURP	35	-0.002	0.029	88	0.001	0.028	0.004	0.63	
EA_SURP × |*EA_SURP*|	35	-0.001	0.004	88	0.000	0.003	0.001	1.10	
EA_LOSS	35	0.143	0.355	88	0.148	0.357	0.005	0.07	

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TABLE 4.13 - *Continued*

Panel B.2: Good News Constant Derivative Matched Subsample

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
MF_SURP	36	0.002	0.004	70	0.004	0.011	0.002	1.53
OCFVOL	36	0.051	0.035	70	0.053	0.036	0.002	0.31
MF_SURP × |*MF_SURP*|	36	0.000	0.000	70	0.000	0.001	0.000	1.59
MF_LOSS	36	0.000	0.000	70	0.000	0.000	0.000	0.00
MF_WIDTH	36	0.002	0.004	70	0.002	0.004	0.000	0.49
MF_HORIZON	36	4.990	0.656	70	4.583	0.954	-0.408	-2.58 *
MTB	36	3.700	4.383	70	2.386	22.444	-1.314	-0.47
MVE	36	6.787	1.620	70	7.182	1.700	0.395	1.17
EA_CONCUR	36	0.611	0.494	70	0.543	0.502	-0.068	-0.67
EA_SURP	22	0.005	0.058	38	0.005	0.018	0.000	0.02
EA_SURP × |*EA_SURP*|	22	0.001	0.012	38	0.000	0.001	-0.001	-0.41
EA_LOSS	22	0.045	0.213	38	0.079	0.273	0.033	0.53

Panel B.3: Bad News Constant Derivative Matched Subsample

	Control			Treat			Difference in Means	
Variables	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Diff.	T-stat
MF_SURP	34	-0.009	0.020	79	-0.006	0.008	0.003	0.81
OCFVOL	34	0.050	0.035	79	0.037	0.025	-0.013	-1.91
MF_SURP × |*MF_SURP*|	34	0.000	0.002	79	0.000	0.000	0.000	1.33
MF_LOSS	34	0.029	0.171	79	0.000	0.000	-0.029	-1.00
MF_WIDTH	34	0.003	0.005	79	0.002	0.003	-0.001	-0.97
MF_HORIZON	34	4.700	0.779	79	4.441	1.061	-0.259	-1.45
MTB	34	3.155	1.866	79	5.608	26.039	2.454	0.83
MVE	34	6.675	1.663	79	7.798	1.860	1.122	3.17 **
EA_CONCUR	34	0.471	0.507	79	0.570	0.498	0.099	0.96
EA_SURP	16	-0.002	0.016	45	-0.006	0.018	-0.004	-0.76
EA_SURP × |*EA_SURP*|	16	0.000	0.000	45	0.000	0.001	0.000	-1.23
EA_LOSS	16	0.125	0.342	45	0.133	0.344	0.008	0.08

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This table reports the descriptive statistics of management forecast and firm characteristic variables, by control and treatment firms, as well as the results of the t-test of means in the constant derivative samples (table 4.4). Panels A.1, A.2 and A.3 (B.1, B.2 and B.3) provide the descriptives and t-test results for the combined, good news and bad news constant derivative unmatched (matched) sample/subsamples, respectively. For ease of interpretation, the summary statistics for *EA_SURP, EA_SURP* × |*EA_SURP*| and *EA_LOSS* are based only on management forecasts that are issued concurrently with an earnings announcement. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the two-tailed tests. All variables are defined in Appendix A.

TABLE 4.14

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Constant Derivative Sample using Alternative Set of Control Variables (Test of H1)

Panel A: Management forecast response coefficient, not conditioned on sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$	
	Predicted	Constant Derivative Unmatched Sample	Constant Derivative Matched Sample
Variable	Sign	(1)	(2)
TREAT	?	0.003	-0.017
		[0.17]	[-0.75]
POST	?	0.034 *	0.042 *
		[1.95]	[1.71]
TREAT × POST	?	-0.008	0.006
		[-0.42]	[0.21]
MF_SURP	+	6.784 ***	1.778 ***
		[3.81]	[4.30]
TREAT × MF_SURP	?	-2.344	2.065
		[-1.57]	[1.36]
POST × MF_SURP	+	1.469	2.543 ***
		[1.02]	[4.42]
TREAT × POST × MF_SURP	-	2.203	-1.412
		[1.08]	[-0.86]
HiOCFVOL	?	-0.017 **	
		[-2.34]	
HiOCFVOL × MF_SURP	-	-2.632 **	
		[-1.86]	
MF_LOSS	?	0.006	
		[0.25]	
MF_LOSS × MF_SURP	-	-2.466 **	
		[-2.15]	
HiMVE	?	-0.010	-0.014
		[-1.18]	[-1.15]
HiMVE × MF_SURP	-	-1.358	-2.942 **
		[-1.01]	[-2.17]
EA_CONCUR	?	0.009	
		[1.30]	
EA_CONCUR × MF_SURP	?	-1.914 **	
		[-2.17]	
Constant	?	-0.019	-0.025
		[-1.26]	[-1.28]
N		667	219
Adjusted R^2		0.177	0.167
df_m		15	9
df_r		230	78

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Panel B: Management forecast response coefficient, conditioned on sign of *MF_SURP*

		DV: *MF_CAR* $_{0,1}$	
		Constant Derivative	Constant Derivative
	Predicted	Unmatched Sample	Matched Sample
Variable	Sign	(1)	(2)
TREAT	?	-0.010	-0.033
		[-0.65]	[-1.18]
POST	?	0.034 *	0.036
		[1.85]	[1.30]
TREAT × POST	?	-0.016	0.028
		[-0.80]	[0.80]
MF_SURP_GNEWS	+	2.964	-0.829
		[0.95]	[-0.24]
MF_SURP_BNEWS	+	5.351 ***	1.758 ***
		[2.50]	[4.22]
TREAT × MF_SURP_GNEWS	?	7.629 **	8.182 *
		[1.98]	[1.89]
TREAT × MF_SURP_BNEWS	?	-2.772 *	-2.895
		[-1.69]	[-1.15]
POST × MF_SURP_GNEWS	+	4.459	12.405 *
		[0.87]	[1.63]
POST × MF_SURP_BNEWS	+	-0.433	1.608 ***
		[-0.29]	[2.61]
TREAT × POST × MF_SURP_GNEWS	-	0.797	-13.770 *
		[0.16]	[-1.52]
TREAT × POST × MF_SURP_BNEWS	-	1.311	0.884
		[0.66]	[0.22]
HiOCFVOL	?	-0.013 *	
		[-1.73]	
HiOCFVOL × MF_SURP_GNEWS	-	-4.620 **	
		[-2.12]	
HiOCFVOL × MF_SURP_BNEWS	-	-1.888	
		[-1.22]	
MF_HORIZON	?	0.002	0.023
		[0.22]	[1.64]
MF_HORIZON × MF_SURP_GNEWS	-	-1.570	0.838
		[-1.03]	[0.29]
MF_HORIZON × MF_SURP_BNEWS	-	0.296	3.296 *
		[0.32]	[1.66]
HiMVE	?	0.008	0.001
		[1.03]	[0.08]

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TABLE 4.14 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)	DV: $MF\_CAR_{0,1}$ Constant Derivative Matched Sample (2)
HiMVE × MF_SURP_GNEWS	-	-7.666 ***	-6.123 ***
		[-3.39]	[-2.81]
HiMVE × MF_SURP_BNEWS	-	0.111	1.335
		[0.08]	[0.42]
Constant	?	-0.018	-0.041 *
		[-1.08]	[-1.71]
N		667	219
Adjusted R^2		0.194	0.17
df_m		20	17
df_r		230	78

This table reports the results of the additional analysis of H1, using the constant derivative samples (table 4.4) and an alternative set of control variables that statistically differ between control and treatment groups within the respective constant derivative samples (table 4.13). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.15

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Constant Derivative Sample using Alternative Set of Control Variables, Additional Analyses (Test of H1)

		DV: $MF\_CAR_{0,1}$		
		Constant Derivative Unmatched Sample	Constant Derivative Unmatched Sample (Exclude Loss Forecasts)	Constant Derivative Matched Sample (Exclude Loss Forecasts)
Variable	Predicted Sign	(1)	(2)	(3)
TREAT	?	-0.009	-0.008	-0.032
		[-0.54]	[-0.46]	[-1.17]
POST	?	0.025	0.019	0.030
		[1.30]	[0.95]	[1.10]
TREAT × POST	?	-0.007	-0.002	0.033
		[-0.34]	[-0.11]	[0.95]
MF_SURP_GNEWS	+	2.703	2.417	-0.539
		[0.79]	[0.77]	[-0.16]
MF_SURP_BNEWS	+	4.381 **	3.186 *	1.695 ***
		[1.96]	[1.45]	[4.11]
TREAT × MF_SURP_GNEWS	?	7.815 *	8.232 **	8.020 *
		[1.90]	[2.11]	[1.87]
TREAT × MF_SURP_BNEWS	?	-2.446	-2.242	-3.834
		[-1.42]	[-1.45]	[-1.61]
POST × MF_SURP_GNEWS	+	19.202 ***	22.487 ***	13.694 **
		[2.81]	[3.04]	[1.81]
POST × MF_SURP_BNEWS	+	1.495	-0.166	-1.638
		[0.77]	[-0.10]	[-0.74]
TREAT × POST × MF_SURP_GNEWS	-	-14.492 **	-17.666 ***	-14.828 *
		[-2.11]	[-2.39]	[-1.65]
TREAT × POST × MF_SURP_BNEWS	-	-0.757	0.333	2.936
		[-0.32]	[0.16]	[0.85]
HiOCFVOL	?	-0.013 *	-0.011	
		[-1.71]	[-1.52]	
HiOCFVOL × MF_SURP_GNEWS	-	-4.345 **	-4.479 **	
		[-2.01]	[-2.06]	

Continued on next page

TABLE 4.15 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)	Constant Derivative Unmatched Sample (Exclude Loss Forecasts) (2)	Constant Derivative Matched Sample (Exclude Loss Forecasts) (3)
HiOCFVOL × MF_SURP_BNEWS	-	-0.919	-0.111	
		[-0.56]	[-0.06]	
MF_LOSS	?	0.072 ***		
		[2.65]		
MF_LOSS × MF_SURP_GNEWS	-	-21.337 ***		
		[-3.76]		
MF_LOSS × MF_SURP_BNEWS	-	-1.656 *		
		[-1.29]		
MF_HORIZON	?	0.001	0.003	0.027 *
		[0.15]	[0.37]	[1.97]
MF_HORIZON × MF_SURP_GNEWS	-	-0.876	-0.981	0.477
		[-0.54]	[-0.60]	[0.17]
MF_HORIZON × MF_SURP_BNEWS	-	0.17	1.165	5.636 **
		[0.17]	[0.87]	[1.99]
HiMVE	?	0.011	0.014 *	0.003
		[1.44]	[1.78]	[0.22]
HiMVE × MF_SURP_GNEWS	-	-7.953 ***	-8.058 ***	-6.224 ***
		[-3.48]	[-3.54]	[-2.88]
HiMVE × MF_SURP_BNEWS	-	1.381	2.449 *	1.973
		[0.97]	[1.37]	[0.62]
Constant	?	-0.021	-0.023	-0.044 *
		[-1.24]	[-1.41]	[-1.85]
N		667	656	218
Adjusted R^2		0.213	0.205	0.135
df_m		23	20	17
df_r		230	228	78

Continued on next page

TABLE 4.15 - *Continued*

This table reports the results of the additional analysis of H1, conditioned on the sign of *MF_SURP*, using the constant derivative samples (table 4.4) and an alternative set of control variables. The regression includes a set of control variables that statistically differ between control and treatment firms within the respective constant derivative samples (table 4.14). The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. Column (1) reports the results including the alternative set of control variables, plus control variables for *MF_LOSS* and its interactions with the forecast surprise variables. Columns (2) and (3) report the results for the constant derivative unmatched and matched samples, after excluding 11 and 1 loss forecast observations, respectively. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE 4.16

Industry Distribution - H2

	Unmatched Sample				Matched Sample			
	All		*Treatment*		*All*		*Treatment*	
Industry	N	% Sample	N	% Industry	N	% Sample	N	% Industry
Consumer Non-Durables	86	6.2	44	51.2	44	7.5	22	50.0
Consumer Durables	42	3.1	15	35.7	12	2.0	6	50.0
Manufacturing	201	14.6	95	47.3	114	19.4	57	50.0
Energy and Extraction	69	5.0	44	63.8	34	5.8	17	50.0
Chemicals and Allied Products	40	2.9	19	47.5	12	2.0	6	50.0
Business Equipment	292	21.2	70	24.0	98	16.7	49	50.0
Telecommunications	23	1.7	10	43.5	6	1.0	3	50.0
Utilities	57	4.1	42	73.7	24	4.1	12	50.0
Wholesale and Retail	153	11.1	50	32.7	82	13.9	41	50.0
Healthcare	199	14.5	34	17.1	52	8.8	26	50.0
Other	215	15.6	69	32.1	110	18.7	55	50.0
Total	1,377	100.0	492	35.7	588	100.0	294	50.0

This table reports the industry distribution of the unmatched and matched H2 samples (table 3.4, panel B), using unique firm observations. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-period. Industry classifications are based on the Fama and French 12 industry classifications.

TABLE 4.17

Pre-Treatment Comparison of Potential Confounding Variable Means - H2

Panel A: Unmatched Sample

	Control (N = 885)		Treat (N = 492)		Difference in Means	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	0.112	0.176	0.053	0.043	-0.058	-7.22 ***
MVE	4.367	1.707	6.293	1.982	1.926	18.92 ***
ANALYSTS_N	1.072	0.978	1.947	1.002	0.876	15.78 ***

Panel B: Matched Sample

	Control (N = 294)		Treat (N = 294)		Difference in Means	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	0.063	0.046	0.059	0.046	-0.003	-0.91
MVE	5.390	1.690	5.531	1.659	0.140	1.02
ANALYSTS_N	1.506	1.021	1.649	0.964	0.143	1.74

Panel C: Positive Intraperiod Return Matched Subsample

	Control (N = 66)		Treat (N = 66)		Difference in Means	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	0.065	0.047	0.068	0.050	0.002	0.26
MVE	6.342	1.743	6.414	1.762	0.072	0.24
ANALYSTS_N	2.174	0.864	2.178	0.822	0.004	0.03

Panel D: Negative Intraperiod Return Matched Subsample

	Control (N = 228)		Treat (N = 228)		Difference in Means	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	T-stat
OCFVOL	0.062	0.046	0.057	0.045	-0.005	-1.20
MVE	5.115	1.574	5.275	1.540	0.160	1.10
ANALYSTS_N	1.313	0.982	1.496	0.949	0.183	2.02 *

This table compares the potential confounding variable means between control and treatment groups for the H2 samples in the pre-period (1999). Panels A and B report the results of the t-test of means for the unmatched and matched samples, respectively. Panels C and D report the results of the t-test of means for the positive and the negative intraperiod return matched subsamples, respectively. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. Treatment firms are those that use derivatives in the pre-period and control firms are those that do not use derivatives in the pre-period. All variables are defined in Appendix A.

TABLE 4.18

The Effect of Exposure to Fair Value Accounting on the Timeliness of Price Discovery (Test of H2)

Panel A: Unmatched Sample

	Control				Treat				
Sample/Subsample	N	Pre *IPT*	Post *IPT*	*ΔIPT*	N	Pre *IPT*	Post *IPT*	*ΔIPT*	*DiD_IPT*
Combined	885	3.942	6.038	2.096 **	492	5.114	6.910	1.796 **	-0.300
Positive intraperiod return	274	3.677	6.038	2.360 *	123	5.518	6.182	0.664	-1.697 *
Negative intraperiod return	611	4.734	6.149	1.415	369	4.323	7.614	3.291 **	1.876 ***

Panel B: Matched Sample

	Control				Treat				
Sample/Subsample	N	Pre *IPT*	Post *IPT*	*ΔIPT*	N	Pre *IPT*	Post *IPT*	*ΔIPT*	*DiD_IPT*
Combined	294	4.227	6.288	2.061	294	4.868	6.696	1.828 *	-0.233
Positive intraperiod return	66	4.095	5.813	1.717	66	5.247	6.338	1.091	-0.626
Negative intraperiod return	228	4.504	6.977	2.473 **	228	4.373	6.997	2.623 *	0.150

This table presents the results of the DiD tests for H2 using the portfolio-level *IPT*. Panels A and B present the results using the unmatched and matched H2 samples, respectively. The matched sample is identified using CEM on *OCFVOL* (28 cutpoints), *MVE* (8 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *ΔIPT* is the post-period *IPT* minus the pre-period *IPT,* within the respective group. *DiD_IPT* is equal to the treatment group *ΔIPT* minus the control group *ΔIPT*. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels based on the one-tailed tests, respectively. Significance of *ΔIPT* and *DID_IPT* is determined using a permutation test, where I compare the observed *ΔIPT* and *DID_IPT* to the null distributions of *ΔIPT* and *DID_IPT* created under the null hypothesis that the order of the monthly returns does not matter. The significance of *ΔIPT* and *DiD_IPT* depend on the respective null distributions, which are unique to the portfolio(s) examined. The corresponding null distributions of *ΔIPT* and *DID_IPT* are presented in appendix E.1. All variables are defined in Appendix A.

TABLE 4.19

Constant Derivative Samples – H2

Panel A: Unmatched Sample

	Number of Firm-Years			Number of Firms		
	Control	Treat	Total	Control	Treat	Total
Unmatched H2 sample	1,770	984	2,754	885	492	1,377
Less: firm-years whose derivative use (non-use) in the post-period is inconsistent with classification in latest pre-period	(302)	(54)	(356)	(151)	(27)	(178)
Constant derivative unmatched H2 sample	1,468	930	2,398	734	465	1,199

Panel B: Matched Sample

	Number of Firm-Years			Number of Firms		
	Control	Treat	Total	Control	Treat	Total
Constant derivative unmatched H2 sample	1,468	930	2,398	734	465	1,199
Less: Firms with no matches using CEM on *OCFVOL*, *MVE*, industry and sign of intraperiod return	(1,020)	(482)	(1,502)	(510)	(241)	(751)
Constant derivative matched H2 sample	448	448	896	224	224	448

Panels A and B of this table report the number of firms and their respective firm-years that comprise the constant derivative unmatched and matched H2 samples, respectively. These samples exclude any firms whose derivative use (non-use) in the post-period is inconsistent with the classification in the pre-period . The constant derivative matched sample is identified using CEM on *OCFVOL* (26 cutpoints), *MVE* (10 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched H2 sample. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. All variables are defined in Appendix A.

TABLE 4.20

The Effect of Exposure to Fair Value Accounting on the Timeliness of Price Discovery - Constant Derivative Sample (Test of H2)

Panel A: Constant Derivative Unmatched Sample

	Control				Treat				
Sample/Subsample	N	Pre *IPT*	Post *IPT*	*ΔIPT*	N	Pre *IPT*	Post *IPT*	*ΔIPT*	*DiD_IPT*
Combined	734	3.799	5.935	2.136 **	465	5.104	6.970	1.866 **	-0.270
Positive intraperiod return	225	3.513	5.987	2.474 *	120	5.483	6.190	0.707	-1.768 *
Negative intraperiod return	509	4.761	6.057	1.296	345	4.343	7.731	3.388 **	2.092 ***

Panel B: Constant Derivative Matched Sample

	Control				Treat				
Sample/Subsample	N	Pre *IPT*	Post *IPT*	*ΔIPT*	N	Pre *IPT*	Post *IPT*	*ΔIPT*	*DiD_IPT*
Combined	224	4.107	6.271	2.164 *	224	4.799	7.053	2.254 **	0.089
Positive intraperiod return	50	3.895	6.211	2.317	50	5.103	6.398	1.295	-1.022
Negative intraperiod return	174	4.574	6.102	1.527	174	4.326	7.659	3.333 **	1.806 ***

This table presents the results of the DiD tests for H2 using the portfolio-level *IPT*. Panels A and B present the results using the constant derivative unmatched and matched H2 samples (table 4.19), respectively. The constant derivative samples exclude firms whose derivative use (non-use) in the post-period is inconsistent with the classification in the pre-period. The constant derivative matched sample is identified using CEM on *OCFVOL* (26 cutpoints), *MVE* (10 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched H2 sample. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *ΔIPT* is the post-period *IPT* minus the pre-period *IPT,* within the respective group. *DiD_IPT* is equal to the treatment group *ΔIPT* minus the control group *ΔIPT*. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels based on the one-tailed tests, respectively. Significance of *ΔIPT* and *DID_IPT* is determined using a permutation test, where I compare the observed *ΔIPT* and *DID_IPT* to the null distributions of *ΔIPT* and *DID_IPT* created under the null hypothesis that the order of the monthly returns does not matter. The significance of *ΔIPT* and *DiD_IPT* depend on the respective null distributions, which are unique to the portfolio(s) examined. The corresponding null distributions of *ΔIPT* and *DID_IPT* are presented in appendix E.2. All variables are defined in Appendix A.

TABLE 4.21

The Effect of Exposure to Fair Value Accounting on the Timeliness of Price Discovery - Alternative Matched Samples (Test of H2)

Panel A: Constant Derivative *OCFVOL*-Matched Sample

	Control				Treat				
Sample/Subsample	N	Pre *IPT*	Post *IPT*	*ΔIPT*	N	Pre *IPT*	Post *IPT*	*ΔIPT*	*DiD_IPT*
Combined	349	4.146	5.774	1.628	349	5.042	7.043	2.001 *	0.373
Positive intraperiod return	88	3.999	5.641	1.642	88	5.279	6.498	1.219	-0.423
Negative intraperiod return	261	4.424	6.122	1.698	261	4.559	7.603	3.044 **	1.345 **

Panel B: Constant Derivative *MVE*-Matched Sample

	Control				Treat				
Sample/Subsample	N	Pre *IPT*	Post *IPT*	*ΔIPT*	N	Pre *IPT*	Post *IPT*	*ΔIPT*	*DiD_IPT*
Combined	245	4.525	6.458	1.932 *	245	5.076	6.681	1.605	-0.327
Positive intraperiod return	57	4.502	6.359	1.856	57	5.435	6.429	0.994	-0.863
Negative intraperiod return	188	4.600	6.140	1.540	188	4.365	6.920	2.555 *	1.014

Panel C: Constant Derivative *OCFVOL-ANALYSTS_N*-Matched Sample

	Control				Treat				
Sample/Subsample	N	Pre *IPT*	Post *IPT*	*ΔIPT*	N	Pre *IPT*	Post *IPT*	*ΔIPT*	*DiD_IPT*
Combined	257	4.318	5.891	1.573	257	5.193	7.049	1.856 **	0.283
Positive intraperiod return	66	4.242	5.587	1.345	66	5.546	6.410	0.864	-0.481
Negative intraperiod return	191	4.440	6.349	1.909	191	4.467	7.446	2.978 **	1.069 *

Continued on next page

This table presents the results of the DiD tests for H2 using alternative matched samples. Panels A, B and C present the results using the constant derivative *OCFVOL* -, *MVE-* , and *OCFVOL-ANALYSTS_N-* matched samples, respectively. The constant derivative samples exclude firms whose derivative use (non-use) in the post-period is inconsistent with classification in the pre-period. The constant derivative *OCFVOL* -matched sample is identified using CEM on *OCFVOL* (44 cutpoints), FF12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample (table 4.19, panel A). The constant derivative *MVE* -matched sample is identified using CEM on *MVE* (10 cutpoints), FF12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. The *OCFVOL-ANALYSTS_N* -matched sample is identified using CEM on *OCFVOL* (33 cutpoints), *ANALYSTS_N* (5 cutpoints), FF12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *ΔIPT* is the post-period *IPT* minus the pre-period *IPT,* within the respective group. *DiD_IPT* is equal to the treatment group *ΔIPT* minus the control group *ΔIPT* . ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels based on the one-tailed tests, respectively. Significance of *ΔIPT* and *DID_IPT* is determined using a permutation test, where I compare the observed *ΔIPT* and *DID_IPT* to the null distributions of *ΔIPT* and *DID_IPT* created under the null hypothesis that the order of the monthly returns does not matter. The significance of *ΔIPT* and *DiD_IPT* depend on the respective null distributions, which are unique to the portfolio(s) examined. The corresponding null distributions of *ΔIPT* and *DID_IPT* are presented in appendix E.3. All variables are defined in Appendix A.

TABLE 4.22

The Effect of Exposure to Fair Value Accounting on Management Forecast Frequency

	Control					Treat					
		Pre	Post				Pre	Post			
Sample/Subsample	N	*MF_N*	*MF_N*	*ΔMF_N*		N	*MF_N*	*MF_N*	*ΔMF_N*		*DiD_MF_N*
Combined	224	0.268	0.456	0.188	***	224	0.367	0.592	0.226	***	0.037
Positive intraperiod return	50	0.196	0.748	0.551	***	50	0.390	0.902	0.512	***	-0.040
Negative intraperiod return	174	0.289	0.373	0.084	*	174	0.360	0.504	0.144	**	0.060

This table presents the results of the DiD t-tests for the effect of exposure to fair value accounting on management forecast frequency using the constant derivative matched H2 sample (table 4.19, panel B). The constant derivative samples exclude firms whose derivative use (non-use) in the post-period is inconsistent with classification in the pre-period. The constant derivative matched sample is identified using CEM on *OCFVOL* (26 cutpoints), *MVE* (10 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *MF_N* is equal to the natural log of one plus the number of management forecasts issued in the 12-month period ending 3 months after the fiscal year-end. *ΔMF_N* is the average value in the post-period minus the average value in the pre-period, within the respective group. *DiD_MF_N* is equal to the treatment group *ΔMF_N* minus the control group *ΔMF_N*. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels based on the one-tailed tests, respectively. All variables are defined in Appendix A.

TABLE 4.23

Impact of SFAS 133 on Analyst Following

	Control				Treat				
Sample/Subsample	N	Pre *ANALYSTS_N*	Post *ANALYSTS_N*	*ΔANALYSTS_N*	N	Pre *ANALYSTS_N*	Post *ANALYSTS_N*	*ΔANALYSTS_N*	*DiD_ ANALYSTS_N*
Combined	224	1.441	1.196	-0.246 **	224	1.546	1.279	-0.267 **	-0.021
Positive intraperiod return	50	2.056	2.123	0.067	50	2.112	2.171	0.059	-0.007
Negative intraperiod return	174	1.265	0.929	-0.335 ***	174	1.383	1.022	-0.361 ***	-0.025

This table presents the results of the DiD t-tests of the impact of SFAS 133 on analyst following using the constant derivative matched H2 sample (table 4.19, panel B). The constant derivative samples exclude firms whose derivative use (non-use) in the post-period is inconsistent with classification in the pre-period. The constant derivative matched sample is identified using CEM on *OCFVOL* (26 cutpoints), *MVE* (10 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *ANALYSTS_N* is equal to the natural log of one plus the number of analysts following the firm in the 12-month period ending 3 months after the fiscal year-end. *ΔANALYSTS_N* is the average value in the post-period minus the average value in the pre-period, within the respective group. *DiD_ANALYSTS_N* is equal to the treatment group *ΔANALYSTS_N* minus the control group *ΔANALYSTS_N*. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels based on the two-tailed tests, respectively. All variables are defined in Appendix A.

Appendix A

Variable Description

Variable	Description
Panel A: Management Forecast-Level Variables	
$MF\_CAR_{0,1}$	Cumulative abnormal returns for day 0 to day 1, calculated as the cumulative daily return minus the corresponding size-decile portfolio return, where day 0 is the management forecast (MF) date. When the forecast is released after the close of trading, the MF date is set to be the next trading day.
MF_SURP	Management forecast surprise, equal to the MF EPS minus the mean analyst forecast EPS in the set of analyst forecasts issued 90 to 2 calendar days prior to the MF date, deflated by the share price 2 trading days prior to MF date (pre-MF share price); if the MF EPS is a closed range, I estimate the news using the midpoint of the range.
MF_LOSS	Indicator variable that equals one if the MF EPS is negative, and zero otherwise.
MF_WIDTH	Width of range or point forecast, where the width for a range forecast is equal to the high-end estimate minus the low-end estimate, deflated by the pre-MF share price and the width for a point forecast is zero.
MF_HORIZON	The number of calendar days between the MF date and the corresponding fiscal year-end date.
EA_CONCUR	Indicator variable that equals one if an earnings announcement was issued within 1 trading day of the MF date, and zero otherwise.
EA_SURP	Earnings surprise of concurrent earnings announcement (i.e., earnings announcement issued within 1 trading day of management forecast), equal to the current quarter EPS minus the four quarters ago EPS, deflated by the share price 2 trading days prior to earnings announcement date; if no earnings announcement is issued within 1 trading day of management forecast, variable is equal to zero.
EA_LOSS	Indicator variable that equals one if the concurrent earnings announcement EPS is negative, and zero otherwise.
HiMF_WIDTH	Indicator variable that equals one if *MF_WIDTH* is above median, and zero otherwise.
HiMF_HORIZON	Indicator variable that equals one if *MF_HORIZON* is above median, and zero otherwise.
MF_SURP _GNEWS	Equal to *MF_SURP* where *MF_SURP* is positive, and zero otherwise.

Continued on next page

Appendix A - *Continued*

Variable	Description
MF_SURP _BNEWS	Equal to *MF_SURP* where *MF_SURP* is negative, and zero otherwise.
MF_BNEWS	Indicator variable that equals one if *MF_SURP* is negative, and zero otherwise.
$MF\_CAR_{-1,1}$	Cumulative abnormal returns for day -1 to day 1, calculated as the cumulative daily return minus the corresponding size-decile portfolio return, where day 0 is the management forecast (MF) date. When the forecast is released after the close of trading, the MF date is set to be the next trading day.
Panel B: Firm-Level Variables	
TREAT	Indicator variable that equals one if the firm holds derivative instruments in the latest pre-SFAS 133 period (1999 for H2 and 1998 or 1999 for H1), and zero otherwise.
POST	Indicator variable that equals one for fiscal years ending on or after June 30, 2001, and zero otherwise.
OCFVOL	Four-year standard deviation of operating cash flows scaled by average total assets.
MVE	The natural logarithm of the market value of common equity (*MVE*) at the beginning of the fiscal year.
MTB	Market value of common equity divided by the book value of common equity at the beginning of the fiscal year.
HiOCFVOL	Indicator variable that equals one if *OCFVOL* is above median, and zero otherwise.
HiMTB	Indicator variable that equals one if *MTB* is above median, and zero otherwise.
HiMVE	Indicator variable that equals one if *MVE* is above median, and zero otherwise.
ANALYSTS_N	The natural logarithm of one plus the number of analysts following the firm during the 12-month ended three months after the fiscal year-end.
MF_N	The natural logarithm of one plus the number of management forecasts issued during the 12-month period ended 3 months after the fiscal year-end.

Continued on next page

Appendix A - *Continued*

Variable	Description
Panel C: Timeliness of Price Discovery and Related Variables	
IPT_Firm	Area under the graph of market-adjusted buy-and-hold returns over a 12-month period, plotted as a percentage of the 12-month market-adjusted buy-and-hold return calculated as: $$IPT = \frac{1}{2}\sum_{m=1}^{12}(BH_{m-1} + BH_m)/BH_{12} = \sum_{m=1}^{11}(BH_m / BH_{12}) + 0.5$$ where: BH_m = equals the market-adjusted portfolio buy-and-hold returns from month 1 through m; and BH_{12} = equals the 12-month market-adjusted portfolio buy-and-hold return for the 12-month period ending three months after the fiscal year-end (Butler et al. 2007).
IPT_Port or *IPT*	Area under the graph of market-adjusted portfolio buy-and-hold returns over a 12-month period, plotted as a percentage of the 12-month market-adjusted portfolio buy-and-hold return. It is calculated in a similar fashion as *IPT_Firm*, but uses portfolio returns. Portfolio returns are calculated as the hedge returns one would earn by taking a long position in firms with a positive 12-month buy-and hold return and a short position in firms with a negative 12-month buy-and hold return within the portfolio.
RET_REV_Firm	Extent of return reversals, calculated as: $$RET\_REV = \frac{\sum_{m=1}^{12}\lvert BH_m - BH_{m-1}\rvert}{\lvert BH_{12}\rvert}$$ where, BH_m and BH_{12} are defined above.
RET_REV_Port or *RET_REV*	Extent of return reversals, calculated in a similar fashion as the *RET_RET_Firm*, but uses portfolio returns (as described for *IPT_Port*).

Appendix B

Limitations of the Intraperiod Timeliness Metric

B.1 Introduction

This appendix discusses some limitations with using the intraperiod timeliness (IPT) metric in research. Section B.2 discusses some of the prior literature using firm-level IPT. Section B.3 discusses limitations with using a firm-level IPT for assessing the timeliness of price discovery. Specifically, I find that the firm-level IPT metric, *IPT_Firm*, is well-behaved only under certain conditions. When these conditions are not satisfied, it is difficult to appropriately interpret the IPT results. Section B.4 examines whether such limitations are alleviated with the portfolio-level IPT metric and, finally, section B.5 concludes.

B.2 Prior Literature using Firm-Level IPT

In addition to portfolio-level IPT curves and/or metrics, prior literature examining an intraperiod timeliness construct also uses firm-level IPT metrics.[46] Examples of these studies include Butler et al. (2007), Ball et al. (2012b), Drake, Thornock and Twedt (2017), Gao, Shivakumar and Sidhu (2018), and Chapman, Miller and White (2019). Specifically, these studies use firm-level IPT metrics constructed similarly to that developed by Butler et al. (2007). The portfolio-level IPT, described in section 3.2.3.1 and presented in equation (3.3), is calculated using Butler et al.'s formula, but using portfolio-level returns instead of firm-level returns.

In this section, I briefly discuss the firm-level IPT metrics used in the studies mentioned above. My purpose is not to assess the appropriateness of their use and/or interpretation, but rather to describe the different ways in which the firm-level IPT has been constructed and used.

[46] See section 3.2.3.1 for a discussion of some of the studies examining portfolio-level IPT curves and/or metrics.

There exist circumstances where the firm-level IPT can be useful for interpreting the timeliness of price discovery, which I discuss in section B.3.

Drake et al. (2017) measure firm-level IPT using daily returns over the 10 and 20 days following an earnings announcement. They find that firms whose earnings announcements are covered by professional and semi-professional intermediaries have greater IPT in the 10- and 20-day post-announcement period, relative to firms whose earnings announcements are not covered by such intermediaries. Similarly, Chapman et al. (2019) find that firms with an in-house investor relations officer have a larger 10-day IPT following the earnings announcement than those without. Unlike Drake et al. (2017) and Chapman et al. (2019) who use IPT metrics that span a relatively short period (i.e., 10 or 20 days), Butler et al. (2017), Ball et al. (2012b) and Gao et al. (2017) use IPT metrics that span a much longer period (i.e., 12 months). Butler et al. (2007) develop and use an annual IPT metric, using monthly returns, to assess whether reporting frequency affects IPT. They find that while firms that voluntarily increase reporting frequency from semiannual to quarterly filings increase in IPT, those that mandatorily increase reporting frequency do not. Using a similar annual IPT metric, Ball et al. (2012b) find that banks with trading securities have lower IPT than those without trading securities. Finally, Gao et al. (2017) find that joining the Singapore Exchange's research incentive scheme, which offers sponsored analyst following for previously unfollowed or poorly followed firms, does not improve the firm's IPT.

Relying on the use of firm-level IPT in the above literature, I initially planned to use firm-level IPT in this thesis. However, as I proceeded to examine this metric more carefully, I became aware of issues related to return reversals, which can render this metric inappropriate for interpreting the timeliness of price discovery. I discuss this issue in the following section.

B.3 Limitations of using Firm-Level IPT

In earlier literature examining IPT curves (e.g., Freeman 1987, Alford et al. 1993, Butler et al. 2007), the implicit assumption in using area-under-the-curve metrics is that the IPT curves are generally monotonically increasing. This literature normally examines IPT curves at the portfolio level. I use figure 2 from Butler et al. (2007) as an example (see Figure B.1). In such cases, it is relatively straightforward to conclude that a larger area under the curve represents quicker price discovery. However, I find, in section B.3.1, that firm-level IPT curves often are *non*-monotonic. Even portfolio-level IPT curves can be non-monotonic if extreme firm-level reversals are not sufficiently averaged out, as I discuss in section B.4. Such cases are difficult to interpret. Unfortunately, prior studies examining IPT metrics do not discuss such problems in great detail.

In figure B.2, I illustrate this issue by plotting the IPT curves for three firm-year observations in the set of 8,626 firm-year observations with necessary IPT data from panel A of table 3.4 (hereafter, IPT sample). The green (solid line, Alaska Air Group 1999) curve represents a scenario where the IPT curve is relatively well-behaved. Notice that the curve is generally monotonically increasing between 0% and 100%, with the exception of small reversals in months -5, -4, 1 and 3. Only 4 out of 8,626 firm-year observations in the IPT sample have zero return reversals. In contrast to the green (solid line, Alaska Air Group 1999) curve, the blue (dashed line, Foot Locker 2002) and red (dotted line, RailAmerica 2001) IPT curves are not well-behaved. The blue (dashed line) curve represents a scenario where the *IPT_Firm* is positive and has large return reversals during the period, while the red (dotted line) curve represents a scenario where the *IPT_Firm* is negative and has large return reversals. When large reversals exist, the area under the curve (i.e., *IPT_Firm*) can fall outside the acceptable boundaries of a well-behaved IPT curve, namely 0.5 to 11.5, discussed in section 3.2.3.1. Note that the *IPT_Firm* for the blue (dashed line) and red (dotted line) curves are 16.17 and -11.24, respectively. When *IPT_Firm* is affected by such reversals, it is difficult to interpret the area under the curve as the timeliness of price discovery. For example, we cannot conclude that Foot Locker (2002) has quicker price discovery than Alaska Air Group (1999) because Foot Locker's *IPT_Firm* is inflated by return reversals.

Return reversals can result from large changes in the sign of genuine news, but it can also result when the market corrects its prior over-reaction to news that was later shown to be unreliable (i.e., inaccurate or managed news). For example, Lang and Lundholm (2000) show that some firms substantially increase the frequency of their voluntary disclosures prior to seasoned equity offerings to positively influence share prices by "hyping" the stock. However, upon the stock issuance announcement, the market partially corrects for their prior over-reaction. Market reactions driven by a false perception of reliability and/or accuracy that reverses when the actual reliability and/or accuracy becomes apparent are, by definition, not highly credible. Whether return reversals result from changes in the sign of genuine news or corrections of prior reactions to managed news, IPT does not adequately reflect the timeliness of price discovery in the presence of non-trivial return reversals within the same period.

I note that the idiosyncratic timing of news arrival at the firm level need not always result in return reversals. For example, assume firm A experiences two positive events in a given period and no negative events, and the two positive events occur early in the period. Next, assume firm B experiences the same two positive events and also no negative events, but the two positive events occur later in the period, relative to firm A. In both cases, there will be no return reversals related to genuine news. However, firm A will have a larger IPT than firm B, ceteris paribus, because of the earlier timing of the two positive events. Therefore, even in the absence of return reversals, the random timing of firm-level news arrival can create problems for interpreting the timeliness of price discovery. However, the impact of such random timing on IPT is much smaller in the absence of return reversals than in their presence, because, absent return reversals, the IPT always falls within the acceptable range of 0.5 of 11.5, which I discuss in B.3.1.

Note, then, that if return reversals are trivial during the period examined *and* there are no substantial differences in the news arrival process across the firms examined, a firm-level IPT can still be useful for interpreting the timeliness of price discovery. For example, when examining a short time period (e.g., few days) around specific events (e.g., earnings announcements), return reversals may not have a material effect on results because a shorter time interval provides less opportunity for drastic changes in returns and limits news arrival.[47] Furthermore, focusing on specific event periods controls for the timing of the major news event.

Prior papers, such as Drake et al. (2017) and Gao et al. (2018) acknowledge that the IPT metric suffers from the influence of extreme outliers and noise. To alleviate the impact of such outliers, these studies transform the IPT metric into a ranked variable. While the ranked IPT measure helps to reduce the influence of extreme IPT observations, it still preserves the relative (mis)order of the firm-level IPT. For example, firms with large IPT values above 11.5, such as Foot Locker (2002) in Figure B.2, will still be ranked as very timely, which may not be appropriate.

B.3.1 Return Reversals

[47] I have not examined the extent of return reversals in such a scenario and only describe this scenario for illustrative purposes. Concluding on the appropriateness of using a firm-level IPT in a particular setting requires examining the setting for return reversals.

To better understand the relation between the extent of return reversals and *IPT_Firm*, I create a proxy to capture the extent of return reversals, as follows:

$$RET\_REV = \frac{\sum_{m=1}^{12}|BH_m - BH_{m-1}|}{|BH_{12}|} \tag{B.1}$$

where BH_m equals the buy-and-hold returns from month 1 through *m*, and *m* equals the month(s) passed since the beginning of the 12-month period. BH_{12} equals the 12-month buy-and-hold return. The absolute values will amplify the ratio in the presence of return reversals. If all monthly returns within a given period are in the same direction (i.e., no return reversals), *RET_REV* will equal 1. Hence, *RET_REV* has a lower bound of 1 and an unlimited upper bound. This metric can be created using firm-level or portfolio-level returns. I denote the firm-level (portfolio-level) metric as *RET_REV_Firm (RET_REV_Port)*.

I first examine the correlation between *RET_REV_Firm* and the absolute value of *IPT_Firm* in the IPT sample. I use the absolute value of *IPT_Firm* because return reversals can inflate the magnitude of *IPT_Firm* in either direction, both positive and negative. If all firms have well-behaved curves (i.e., monotonically increasing), I expect the correlation to be zero. To the extent that small return reversals remain, like that of Alaska Air Group (1999) in figure B.2, I expect a small positive correlation. In contrast, if return reversals are substantial, I expect a large positive correlation. I find that the Pearson correlation is 0.9477 (untabulated). Accordingly, *RET_REV_Firm* and the absolute value of *IPT_Firm* are highly positively correlated, indicating that return reversals significantly inflate the magnitude of *IPT_Firm*.

Next, I examine the IPT curves of three different firm-year observations in the IPT sample whose *RET_REV_Firm* falls in each the following regions: (1) 1–2; (2) 2-3; and (3) 3-4. This allows me to assess the extent or severity of return reversals, which is not readily apparent when examining *RET_REV_Firm* values. Figure B.3 presents these IPT curves. The green (solid line) IPT curve has a modest *RET_REV_Firm* of 1.45, which falls below the 10th percentile of the *RET_REV_Firm* distribution, discussed below (table B.1). While return reversals exist, in this case they do not appear severe. For example, the magnitude of the largest reversal, in month -2, is smaller than the magnitude of the prior month's returns.

The blue (dashed line) IPT curve has the largest *RET_REV_Firm* (3.38) out of the IPT curves in this figure, which is close to the median value of the *RET_REV_Firm* distribution in the IPT sample (table B.1). It is apparent that the return reversals are much more severe in the blue (dashed line) curve than in the green (solid line) curve. The magnitude of the largest reversal, in month -1, comprises nearly 50% of the 12-month buy-and-hold return. The return reversals in this curve are salient and the curve is not generally monotonically increasing. When return reversals are of such magnitude, it is problematic to interpret the area under the IPT curve (i.e., *IPT_Firm*) as timeliness of price discovery.

Finally, the extent of return reversals in the red (dotted line) IPT curve falls somewhere in between that of the green (solid line) IPT curve and the blue (dashed line) IPT curve; *RET_REV_Firm* is equal to 2.28. While not as severe as that of the blue (dashed line) IPT curve, the extent of return reversals still warrants caution when interpreting *IPT_Firm* as timeliness of price discovery. In sum, I would consider a *RET_REV_Firm* somewhere between that of the green (solid line) IPT curve and the red (dotted line) IPT curve to be acceptable when using IPT metrics to infer the timeliness of price discovery. Thus, as a general guide, I consider *RET_REV* values above two to be non-trivial and difficult to interpret. While IPT curves with *RET_REV* below two can be considered generally monotonically increasing, one should use caution in interpreting IPT metrics as the speed of price discovery.

Next, in table B.1, I examine the distribution of *IPT_Firm* and *RET_REV_Firm* in the IPT sample. I find that the 10^{th} and 90^{th} percentile *IPT_Firm* (i.e., area under the curve) are well outside the acceptable boundaries of a well-behaved IPT curve, indicating the presence of return reversals - some of which are extreme. Recall that a well-behaved IPT curve begins at 0% and gradually ascends to a peak of 100% at the end of the period; *IPT_Firm* should lie between 0.5 and 11.5. While the *IPT_Firm* values between the 25^{th} and the 75^{th} percentile fall within the acceptable boundaries, I caution that this does not necessarily signal the absence of salient return reversals. To illustrate this point, in figure B.4, I plot the IPT curves of two firm-year observations in the IPT sample whose *IPT_Firm* fall well within 0.5 and 11.5. Note that the blue (solid line) IPT curve has severe return reversals, but due to the negative and positive areas offsetting, the *IPT_Firm* is 5.84. The *RET_REV_Firm* for this firm-year, indeed, is large (19.12), indicating large return reversals. While not as severe as the blue (solid line) IPT curve, the orange (dashed line) IPT curve also contains non-trivial return reversals, with the curve surpassing 100% on the y-axis at month -5 and coming back down to near 0% in month -3. Again, such return reversals are not evident from the *IPT_Firm* value of 5.86. However, the *RET_REV_Firm* of 4.15 indicates that the IPT curve does, indeed, contain non-trivial return reversals.

In table B.1, I find that the minimum *RET_REV_Firm* is 1, indicating that there are some firm-year observations with no return reversals. However, there are only 4 such observations in the IPT sample of 8,626 observations. The majority of the observations have some level of reversals in monthly returns. The 25^{th} percentile *RET_REV_Firm* value is 1.973, indicating that close to 75% of the observations in the IPT sample have non-trivial return reversals, based on my analysis above. Also, note that the 90^{th} percentile *RET_REV_Firm* value is 16.918, which is extreme. The mean absolute *IPT_Firm* for firms with *RET_REV_Firm* above the 90th percentile is 120.176 (untabulated), so the impact of such extreme return reversals on *IPT_Firm* can be huge.

Based on the above, it is important to note that we cannot assess the severity of return reversals in the IPT curve simply by examining the *IPT_Firm* value. As illustrated in figure B.4, an apparently acceptable *IPT_Firm* value can contain large return reversals, which can render the *IPT_Firm* useless for assessing the timeliness of price discovery. The *RET_REV* metric can provide some insight into the extent of return reversals contained in the IPT curve without having to plot the curve itself for every firm-year observation. Specifically, IPT observations with *RET_REV* above a value of two warrant greater attention before the IPT metric can be deemed reliable for interpreting the timeliness of price discovery.

B.4 Portfolio-Level IPT

The portfolio-level IPT, although measured in the same manner as the firm-level IPT, is less problematic because portfolio returns mitigate the effect of idiosyncratic firm-level news arrival, including return reversals, through averaging. Of course, the extent to which such return reversals are averaged out depends on the portfolio having a sufficient number of observations whose return reversals are not synchronous. The prior literature using portfolio IPT metrics does not discuss the minimum portfolio size needed to validate this assumption, but studies have used portfolios as small as 44 observations. For example, Alford et al. (1993) use portfolios ranging from 44 to 2,302 observations and Butler et al. (2007) use portfolios comprised of 98 observations.

To better understand whether using portfolio returns indeed mitigate the problem of extreme return reversals, I examine the *IPT_Port* and *RET_REV_Port* distribution using 1,000 random samples of n observations in the IPT sample, drawn without replacement, where n equals 50, 100, 200, 500 and 1000 observations. *IPT_Port* and *RET_REV_Port* are created in the same manner as *IPT_Firm* and *RET_RET_Firm*, but use portfolio returns, where portfolio returns are equal to the hedge returns one would earn by taking a long position in firms with a positive 12-month buy-and-hold return and a short position in firms with a negative 12-month buy-and-hold return, as discussed in section 3.2.3.1. To the extent extreme reversals are averaged out, I expect to find *IPT_Port* well within the acceptable range of 0.5 to 11.5 across the distribution and *RET_REV_Port* between one and two. Furthermore, if the impact of idiosyncratic timing of news arrival at the firm level is averaged out in portfolios, I expect to observe a small standard deviation in the distribution of *IPT_Port*. As discussed in section B.3, the random timing of news arrival may not always lead to return reversals.

Panels A and B of table B.2 report the *IPT_Port* and *RET_REV_Port* distributions, respectively, across 1000 random portfolios of 50, 100, 200, 500 and 1000 observations. At all portfolio sizes, I find that the entire distribution of *IPT_Port* is within the acceptable range of 0.5 to 11.5 (panel A). As the portfolio size increases, the standard deviation decreases, indicating that the impact of idiosyncratic timing of news arrival decreases as portfolio size increases. Also, I find that the mean *RET_REV_Port* is very close to one (panel B) for all portfolio sizes. Similar to *IPT_Port*, the standard deviation of *RET_REV_Port* decreases as the portfolio sizes increases, indicating that a larger portfolio size better averages away firm-level return reversals. The maximum *RET_REV_Port* across all portfolio sizes is below two. As discussed in section B.3.1, I consider *RET_REV* above two to be problematic. Hence, I posit that *IPT_Port* can be used with caution, to interpret the timeliness of price discovery. In figure B.3, I plotted an IPT curve with *RET_REV_Firm* of 1.45, which I consider non-trivial, but not severe. A portfolio size of 100 observations would limit *RET_REV_Port* to a similar level since the maximum *RET_REV_Port* is 1.411.

I also compare the distribution of *RET_REV_Port* and *IPT_Port* to those of *RET_REV_Firm* and *IPT_Firm* (using values from table B.1). I find that the maximum *RET_REV_Port* is smaller than the 10th percentile *RET_REV_Firm* value for all portfolios with at least 100 observations, indicating a drastic improvement in the monotonicity of the IPT curves with portfolio-level returns, relative to those with firm-level returns.

Based on this analysis, I conclude that *IPT_Port* is more appropriate than *IPT_Firm,* for interpreting the timeliness of price discovery. Portfolio-level returns average away return reversals reasonably well, even at a portfolio size of 50, although it is generally better to use larger portfolio sizes to minimize the impact of return reversals. The mean portfolio with 500 or more observations has zero return reversals; thus, *IPT_Port* can be reliably interpreted as timeliness of price discovery.[48] Portfolios with 50 to fewer than 500 observations can be used to assess intraperiod timeliness, with caution. Furthermore, portfolio-level returns average away the idiosyncratic timing of news *arrival* at the firm level, quite substantially. In tests of H2, I use portfolio sizes ranging from 50 to 885. According to this analysis, these portfolio sizes are reasonable for using *IPT_Port* as a proxy for timeliness of price discovery.

B.5 Conclusion

Based on the above, I do not believe I can appropriately interpret results using a firm-level IPT in my setting. Specifically, IPT curves are generally not well-behaved at the firm level and I find this to be the case in the IPT sample. In contrast, portfolio-level IPT averages out return

[48] Recall from above that *RET_REV* of one is the lower threshold where there are no return reversals.

reversals, and reduces their impact noticeably, even with portfolios of 50 observations, although it is better to use larger portfolios to minimize the impact of return reversals. Furthermore, portfolio-level IPT also averages out the impact of idiosyncratic timing of firm-level news arrival. Hence, I use a portfolio-level IPT metric instead of a firm-level one.

FIGURE B.1
IPT Figure from Butler, Kraft and Weiss (2007)

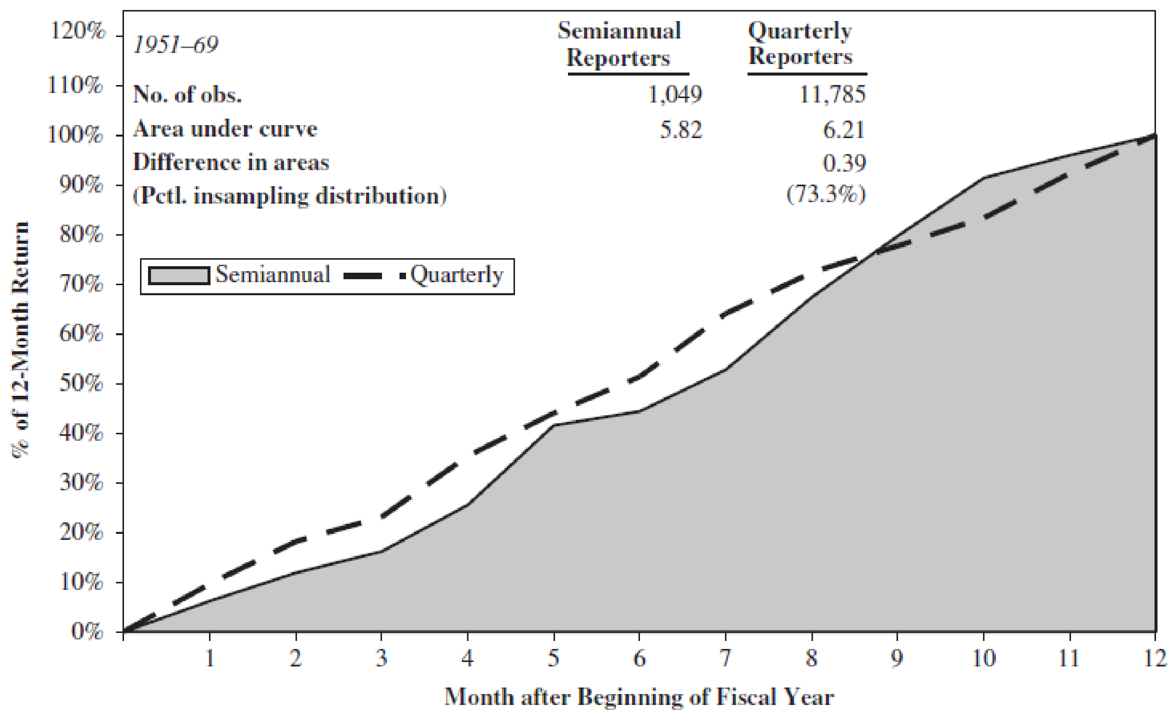

Fig. 2. Timeliness of annual earnings by reporting frequency. Plots reflect percentage of 12-month cumulative returns earned by earnings-based hedge portfolio as of the end of each month during 12-month period after beginning of event-year; that is, for each month m $(m = 1{,}12)$, the plotted percentage is $y_m = EHPRet_m/EHPRet_{12} \times 100$. The earnings-based hedge portfolio comprises a long (short) position in the top (bottom) 27% of firms ranked annually on the change in annual earnings per share scaled by price. Semiannual and quarterly observations are separately pooled across all years, resulting in one hedge portfolio per reporting frequency. Area under graph is $IPT = \frac{1}{2}\sum_{m=1}^{12}(BH_{m-1} + BH_m)/BH_{12} = \sum_{m=1}^{11}(BH_m/BH_{12}) + 0.5$. See the appendix for test statistic details.

FIGURE B.2

Example of IPT_Firm Curves

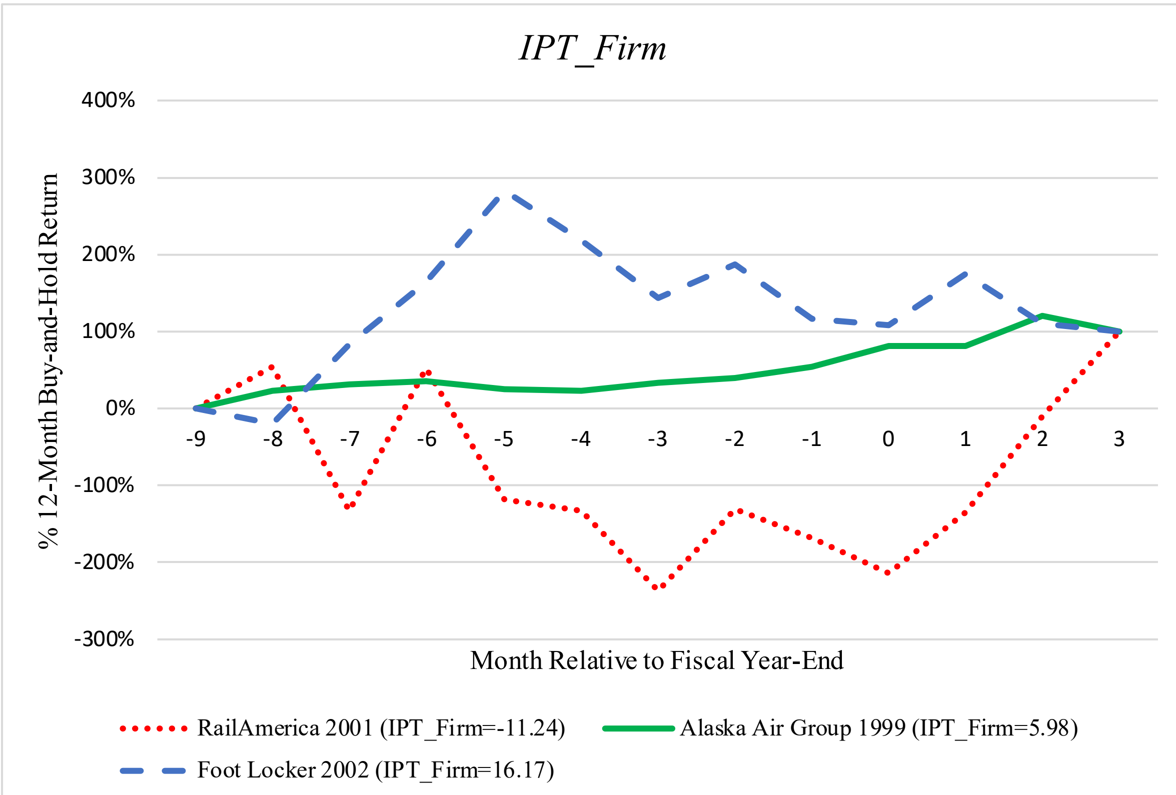

Figure B.2. This figure represents three types of IPT curves, using firm-year observations from the IPT sample (table 3.4, panel A). The green (solid line) curve represents a generally well-behaved IPT curve. The blue (dashed line) curve represents a scenario where the *IPT_Firm* is positive and has large return reversals during the period. The red (dotted line) curve represents a scenario where the *IPT_Firm* is negative and has large return reversals.

FIGURE B.3

Example of IPT_Firm Curves with Varying RET_REV_Firm

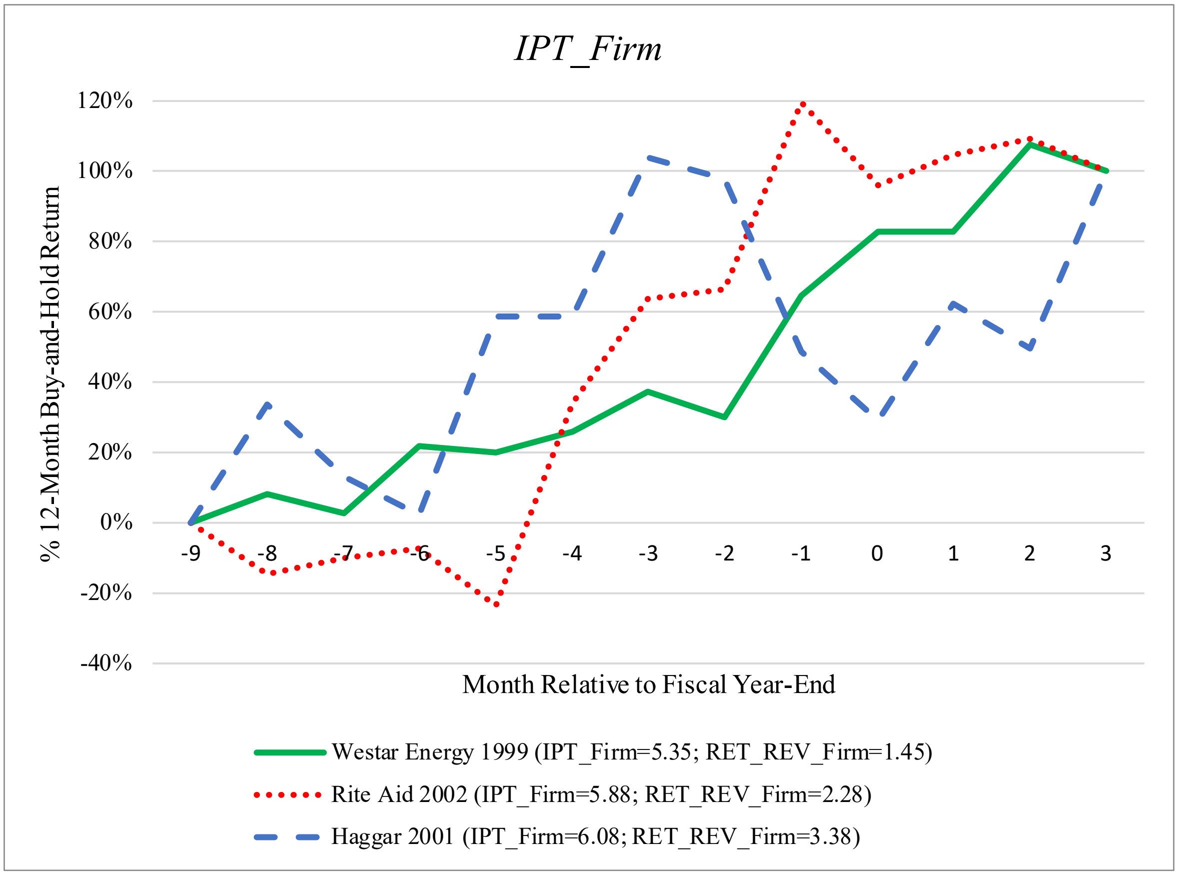

Figure B.3. This figure represents IPT curves for three firm-year observations, with varying levels of *RET_REV_Firm,* in the IPT sample (table 3.4, panel A). The green (solid line) IPT curve has a modest *RET_REV_Firm* of 1.45, which falls below the 10th percentile of the *RET_REV_Firm* distribution in the IPT sample (table B.1). The red (dotted line) IPT curve has a slightly higher *RET_REV_Firm* than that of the green (solid line) IPT curve. Finally, the blue (dashed line) IPT curve has the greatest *RET_REV_Firm* in this graph, which is close to the median value of the *RET_RET_Firm* distribution in the IPT sample (table B.1).

FIGURE B.4

Example of IPT_Firm Curves with Similar IPT_Firm but Substantially Different RET_REV_Firm

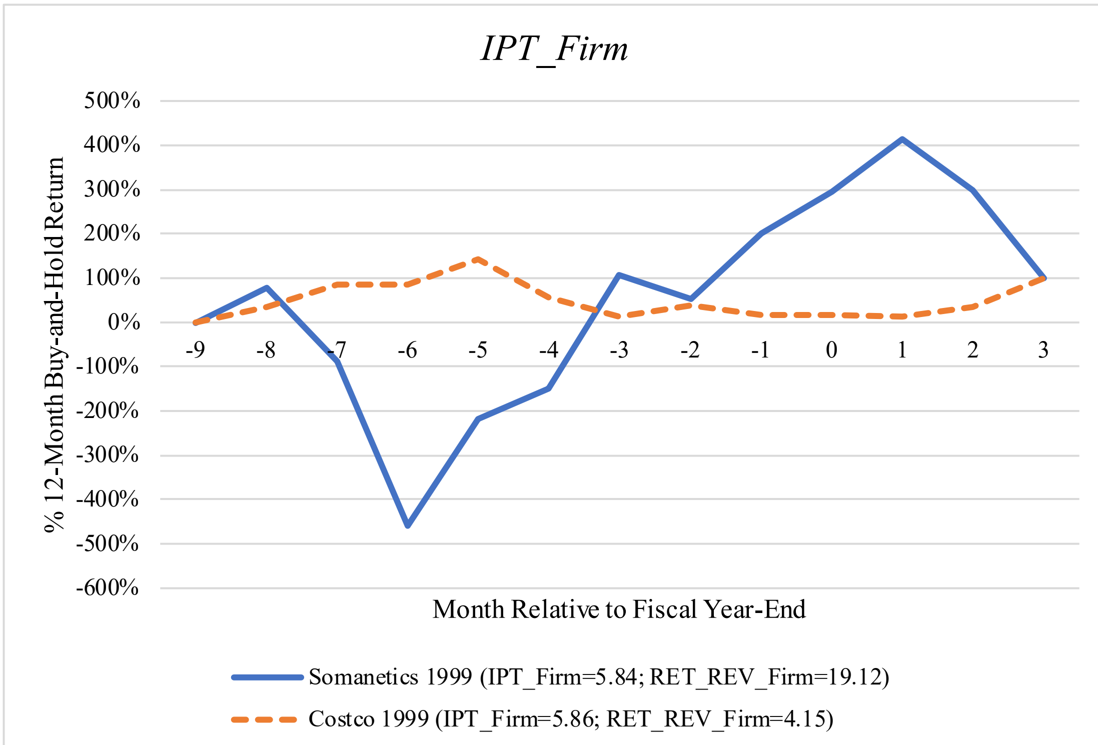

Figure B.4. This figure represents IPT curves for two firm-year observations in the IPT sample (table 3.4, panel A). The *IPT_Firm* for these observations lie well within the interval [0.5, 11.5], where a well-behaved IPT curve can be interpreted. However, these IPT curves contain non-trivial return reversals, making it problematic to interpret the area beneath the curves as timeliness.

TABLE B.1

Distribution of IPT_Firm and RET_REV_Firm in the IPT sample

Variables	N	mean	sd	min	p10	p25	p50	p75	p90	max
IPT_Firm	8626	6.717	140.061	-2644.993	-1.728	2.938	5.422	8.205	13.293	10317.850
RET_REV_Firm	8626	15.323	248.325	1.000	1.484	1.973	3.179	6.531	16.918	20867.220

This table presents the distribution of *IPT_Firm* and *RET_REV_Firm* for the IPT sample of 8,626 firm-year observations (see table 3.4, panel A).

TABLE B.2

Distribution of IPT_Port and RET_REV_Port, by Portfolio Size

Panel A: Distribution of *IPT_Port* using 1,000 random samples of n observations

n	mean	sd	min	p10	p25	p50	p75	p90	max
50	4.826	0.761	2.343	3.909	4.301	4.771	5.291	5.818	7.752
100	4.780	0.543	3.357	4.106	4.408	4.769	5.124	5.476	6.838
200	4.751	0.353	3.625	4.313	4.521	4.730	4.969	5.218	5.999
500	4.727	0.226	4.054	4.454	4.581	4.723	4.871	5.022	5.710
1000	4.730	0.155	4.222	4.535	4.631	4.727	4.833	4.930	5.309

Panel B: Distribution of *RET_REV_Port* using 1,000 random samples of n observations

n	mean	sd	min	p10	p25	p50	p75	p90	max
50	1.066	0.097	1.000	1.000	1.000	1.027	1.091	1.191	1.937
100	1.016	0.039	1.000	1.000	1.000	1.000	1.010	1.057	1.411
200	1.003	0.013	1.000	1.000	1.000	1.000	1.000	1.000	1.138
500	1.000	0.000	1.000	1.000	1.000	1.000	1.000	1.000	1.006
1000	1.000	0.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Panels A and B of this table present the distribution of *IPT_Port and RET_REV_Port,* respectively, using 1,000 random samples of n observations from the IPT sample of 8,626 firm-year observations (table 3.4, panel A), drawn without replacement. The portfolio returns are calculated as the returns one would earn by taking a long position in the positive 12-month buy-and-hold return firms and a short position in in the negative 12-month buy-and-hold return firms.

Appendix C

Identification of Derivative Users and Non-Users

C.1 Introduction

As discussed in section 3.2.1, I use a combination of a keyword search and manual examination of the 10-K filing to identify derivative users and non-users. This appendix describes the identification process and defines the criteria for identifying users and non-users based on the SeekEdgar keyword search results. In section C.2, I discuss the process for identifying derivative users and non-users from the set of latest pre-period firm-year observations available for H1 and/or H2 prior to identifying treatment firms and matching, described in table C.1 and discussed below. Then, in section C.3, I describe the procedures carried out to calibrate and validate the criteria for identifying derivative users using the keyword search results. As discussed in section 3.2.1, I aim to identify derivative users and non-users using the count of keywords and the presence/absence of specific instrument words, without manual verification, whenever it is reasonably feasible to do so.

C.2 Process for Identifying Derivative User and Non-users

To classify firms as derivative users/non-users in the pre-period, I first obtain the count of derivative keywords from the keyword search using the phrase/word count request in SeekEdgar. I use a comprehensive list of 101 derivative-related keywords/phrases (generic and specific instrument words) in figure C.1, similar to that used by Campbell et al. (2020), as discussed in section 3.2.1. I also classify whether a firm has at least one specific instrument word, as discussed in section 3.2.1. Then, I use a combination of the derivative word count and the presence/absence of specific instrument words to identify derivative users and non-users. Where I cannot reasonably classify a firm as a derivative user or non-user based on the keyword results, I manually examine the 10-K filing on SEC's EDGAR database. I discuss cases where I cannot clearly distinguish between derivative users and non-users using the keyword results only, in section C.3.

Table C.1 reports the number of observations included in the process for identifying treatment firms. Panel A reports the SeekEdgar keyword search results. I begin with the set of 1,629 latest pre-period firm-year observations in H1 and/or H2 (hereafter, SeekEdgar sample). This set comprises 294 firms (71 in both the H1 and H2 sets and 223 in the H1 set only) available for H1 prior to identifying treatment firms and matching (see table 3.1, panel B) and 1,406 firms (71 in both the H1 and H2 sets and 1,335 in the H2 set only) available for H2 prior to identifying treatment firms and matching (see table 3.4, panel B).

I include all 1,629 firm-years in the SeekEdgar keyword search. Out of this sample, SeekEdgar does not return results for 110 observations due to missing 10-K filings on the EDGAR database or missing or miscoded headings for the footnote section of the 10-K (i.e., notes to the financial statements), in which case SeekEdgar cannot detect the footnote section. In addition, I do not rely on SeekEdgar results for 13 observations where SeekEdgar reports the total words in the footnote section as less than 1000 words. I trace these observations to the 10-K filings and find that SeekEdgar missed all or part of the notes to the financial statements. This can lead to unreliable results for the number of derivative words.[49] Thus, the SeekEdgar keyword search provides appropriate results for 1,506 pre-period firm-year observations

C.3 Criteria to Identify Derivative Users and Non-users Using Keyword Search Results

I examine the SeekEdgar keyword search results in the set of 1,506 observations to determine the criteria for classifying a firm as a derivative user or non-user. The starting point for this analysis is based on Campbell et al.'s (2020) classification strategy, where they classify a firm as a user if it has 20 or more derivative-related keywords. All other firms are then classified as a non-user. They then manually verify observations with 20 or more derivative words to ensure they are indeed derivative users. In contrast, I want to rely solely on the keyword search results, without manual verification, to identify derivative users and non-users for at least a subset of firms where this can be reasonably done, as discussed in section 3.2.1. Therefore, I need to understand the relation between the derivative word count, the presence/absence of specific instrument words and derivative use.

To gain this understanding, I examine the 10-K filings for random samples of 50 firms in each of the following categories, as identified by the SeekEdgar results: (1) 1-5 derivative words; (2) 6-10 derivative words; (3) 11-15 derivative words; (4) 16-19 derivative words; and (5) 20-30 derivative words. The last group begins with 20 words, instead of 21, to keep consistent with the threshold used in Campbell et al. (2020). Based on their initial identification criteria of 20 or more derivative words, I assume that observations with greater than 30 derivative words are

[49] I find that 9 out of the 13 observations have at least one derivative word, but the SeekEdgar results for all 13 observations indicated zero derivative words.

unambiguously derivative users.[50] Thus, I cut off the analysis at 30 derivative words. Similar to the manual identification of derivative users and non-users, described in section 3.2.1, I search for the keywords - derivative, hedge, hedging, and swap - and read the surrounding text to assess whether the firm uses derivatives. I specifically search for the word “swap” because interest rate swaps are not always described as derivatives.

Table C.2 provides the results of this analysis. Based on these results, I establish the criteria for classifying firms as derivative users and as derivative non-users. I first assess whether the category (e.g., 1-5 derivative words, 6-10 derivative words, etc.) can identify derivative users or non-users reasonably well. To minimize the impact of mis-classification errors, especially given the small sample sizes in this thesis, I only accept classifications with an error rate of 2% or less. I also assess whether the presence/absence of specific instrument words improves the classification error rate since the presence of specific instrument words is more likely to identify derivative users. Indeed, I find that 10.7% (8/75) of derivative non-users mention specific instrument words while 85.1% (149/175) of derivative users mention these words (see table C.2). When the presence/absence of specific instrument words improves the classification error rate and the error rate is 2% or less, I rely on the criteria using both the derivative word count and the presence/absence of specific instrument words to minimize classification errors.

In the random sample of 50 observations with 1-5 derivative words, there are 14 derivative users (table C.2). Hence, a classification scheme based on 1-5 derivative word count

[50] I find that all 50 random observations manually examined in the 20-30 derivative words category are derivative users (see table C.2).

would have a high false negative rate (28% - 14/50).[51] Incorporating the presence/absence of specific instrument words improves the error rate; the false positive rate is 6% (3/50) and the false negative rate is 18% (9/50). That is, in firms with 1-5 derivative words *and* the presence (absence) of specific instrument words, there are 3 (9) derivative non-users (users) out of the 50 observations examined. Hence, I cannot reasonably distinguish derivative users from non-users when observations have 1-5 derivative words as the error rates are greater than 2%.

Next, I examine the category with 6-10 derivative words. A false negative rate is even higher in this category (40% - 20/50), relative to the 1-5 derivative words category, because there is a more even distribution of derivative users and non-users in this category. Incorporating the presence/absence of specific instrument words improves the false negative rate (14% - 7/50). The false positive rate is 6% (3/50). In the category of 11-15 derivative words, the false positive error rate is 16% (8/50). The false positive rate falls to 4% (2/50) once I consider the presence/absence of specific instrument words. The false negative rate is also 4% (2/50). In both categories, the error rates are still above the desired rate of 2%.

In the category of 16-19 derivative words, the false positive error rate is 2% (1/50), which falls within the desired error rate of 2% or less. Nevertheless, I consider whether incorporating the presence/absence of specific instrument words improves the error rate even further. It does for the category of firms with specific instrument words. Specifically, the false positive error rate reduces to 0% (0/50). The false negative error rate is, however, 6% (3/50). Thus, I classify all

[51] A false positive (negative) rate, in this context, refers to the percentage of firms wrongly identified as derivative users (non-users). Incorporating the presence/absence of specific instrument words, a false positive (negative) rate refers to the percentage of firms with (without) specific instrument words that are wrongly identified as derivative users (non-users).

firms with 16-19 derivative words and at least one specific instrument word as derivative users. In contrast, I cannot reasonably classify firms with 16-19 derivative words and no specific instrument words as derivative users or non-users using only the SeekEdgar results. However, only four firms in the SeekEdgar sample have 16-19 derivative words and no specific instrument words and all four firms were included in the manual verification sample. So, no further manual data collection is required for the category of firms with 16-19 derivative words.

Finally, all 50 random observations with 20 to 30 derivative-related words are derivative users, regardless of the presence/absence of specific instrument words, resulting in a 0% false positive rate. Hence, I classify all observations with 20 or more derivative words as derivative users.

In sum, I identify a firm as a derivative user if it has 20 or more derivative words, or 16-19 derivative words *and* at least one specific instrument words. I identify a firm as derivative non-users if it has zero derivative words. I do not classify other observations (i.e., 1-15 derivative words; 16-19 derivative words *and* no specific instrument words), as users or non-users. Out of the 1,506 pre-period firm-years with appropriate SeekEdgar results, I can classify 739 observations as derivative users or non-users (i.e., treatment) using the derivative word count and presence/absence of specific instrument words (table C.1, panel A).

For the remaining 767 observations (table C.1, panel A), the SeekEdgar results are ambiguous for assessing treatment. Hence, for these 767 observations, plus the 110 observations with no SeekEdgar results and 13 observations with inappropriate SeekEdgar results, I classify as a derivative user or non-user by manually examining the 10-K filing. Panel B of table C.1 reports the observations for which I assess treatment by manually tracing to the 10-K filing. Of the 890 firm-year observations needing manual assessment, I can assess treatment (i.e., derivative user or non-user) for 854 observations. I cannot find the corresponding 10-K filing on SEC's EDGAR database for 36 firm-years.[52] Thus, these observations are excluded from the H1 and H2 samples (see table 3.1, panel B and table 3.4, panel B). In sum, the final set of observations where I can assess treatment includes 1,593 observations (table C.1, panel C), comprising 739 observations where I assess treatment using the SeekEdgar results and 854 observations where I assess treatment manually.

[52] All of these 36 observations are part of the 110 observations with no SeekEdgar results (table 3.1, panel B).

FIGURE C.1

Derivative-Related Keywords/Phrases

Generic derivative-related keywords/phrases

barrier option(s), basket option(s), call contract(s), cap agreement(s), compound option(s) contracts are designated, derivative(s), forward contract(s), hedge(s), hedging, ineffective portion(s), instruments are designated, lock agreement(s), lookback option(s), notional, option contract(s), put contract(s), put option(s), ratchet option(s), swap(s), swaption(s), quanto(s).[53]

Specific derivative instrument-related keywords/phrases

average rate option(s), commodity cap(s), commodity contract(s), commodity derivative(s), commodity floor(s), commodity forward(s), commodity future(s), commodity instrument(s), commodity option(s), commodity price cap(s), commodity price contract(s), commodity price collar(s), commodity price derivative(s), commodity price instrument(s), commodity price floor(s), commodity price forward(s), commodity price future(s), commodity price option(s), commodity price swap(s), commodity swap(s), cross currency basis swap(s), cross currency swap(s), currency cap(s), currency collar(s), currency contract(s), currency derivative(s), currency floor(s), currency forward(s), currency future(s), currency instrument(s), currency rate cap(s), currency rate collar(s), currency rate contract(s), currency rate derivative(s), currency rate floor(s), currency rate forward(s), currency rate future(s), currency rate instrument(s), currency rate option(s), currency rate swap(s), currency swap(s), equity swap(s), foreign exchange cap(s), foreign exchange collar(s), foreign exchange contract(s), foreign exchange derivative(s), foreign exchange floor(s), foreign exchange forward(s), foreign exchange future(s), foreign exchange instrument(s), foreign exchange option(s), foreign exchange rate cap(s), foreign exchange rate collar(s), foreign exchange rate contract(s), foreign exchange rate derivative(s), foreign exchange rate floor(s), foreign exchange rate forward(s), foreign exchange rate future(s), foreign exchange rate instrument(s), foreign exchange rate option(s), foreign exchange rate swap(s), foreign exchange swap(s), forward foreign exchange(s), forward rate agreement(s), forward rate contract(s), forward rate option(s), interest rate cap(s), interest rate collar(s), interest rate contract(s), interest rate derivative(s), interest rate floor(s), interest rate forward(s), interest rate future(s), interest rate instrument(s), interest rate lock(s), interest rate option(s), interest rate swap(s), single currency basis swap(s), zero coupon swap(s).

[53] I remove duplicate counts of "derivatives" and "swap(s)", where such words are part of specific derivative instrument-related phrases, such as "commodity derivative(s)", "interest rate derivative(s)", "interest rate swap(s)", and "foreign exchange rate swap(s)". I also run a separate search for the phrase "derivatives litigation" and "hedge fund" to adjust for instances where the keywords do not relate to derivative use.

TABLE C.1

Identification of Treatment Firms – Number of Latest Pre-Period Firm-Year Observations

Panel A: SeekEdgar keyword search results

	H1 & H2	H1 only	H2 only	Total
Set of available observations in H1 and/or H2 prior to identifying treatment firms and matching (SeekEdgar sample)	71	223	1,335	1,629
Less: Observations with no SeekEdgar results	(6)	(18)	(86)	(110)
Less: Observations with inappropriate SeekEdgar results (i.e., <1000 total words in the footnote section of the 10-K)	-	-	(13)	(13)
Set of observations with appropriate SeekEdgar results	65	205	1,236	1,506
Less: Observations where SeekEdgar results are ambiguous for assessing treatment	(24)	(78)	(665)	(767)
Set of observations where I assess treatment using SeekEdgar results	41	127	571	739

Panel B: Manual identification of derivative users and non-users

	H1 & H2	H1 only	H2 only	Total
Observations with no SeekEdgar results	6	18	86	110
Observations with inappropriate SeekEdgar results	-	-	13	13
Observations where SeekEdgar results are ambiguous for assessing treatment	24	78	665	767
Set of observations needing manual assessment	30	96	764	890
Less: Observations with no corresponding 10-K filing	(3)	(7)	(26)	(36)
Set of observations where I assess treatment manually	27	89	738	854

Panel C: Set of observations where I can assess treatment

	H1 & H2	H1 only	H2 only	Total
Set of observations where I assess treatment using SeekEdgar results	41	127	571	739
Set of observations where I assess treatment manually	27	89	738	854
Set of observations where I can assess treatment	68	216	1,309	1,593

This table reports the number of observations included in the process for identifying treatment firms. Panel A reports the observations where I can assess treatment using SeekEdgar keyword search results. Panel B reports the observations where I can assess treatment by manually tracing to the 10-K filing. Panel C reports the set of observations where I can assess treatment. Panel A begins with the set of 294 firms (71 in both the H1 and H2 sets and 223 in the H1 set only) available for H1 prior to identifying treatment firms and matching (see table 3.1, panel B) and 1,406 firms (71 in both the H1 and H2 sets and 1,335 in the H2 set only) available for H2 prior to identifying treatment firms and matching (see table 3.4, panel B) .

TABLE C.2

Identifying Derivative Users Using SeekEdgar Results - Random Samples

1-5 Derivative words

	Derivative user		
Specific instrument words	N	Y	Total
Y	3	5	8
N	33	9	42
Total	36	14	50

6-10 Derivative words

	Derivative user		
Specific instrument words	N	Y	Total
Y	3	13	16
N	27	7	34
Total	30	20	50

11-15 Derivative words

	Derivative user		
Specific instrument words	N	Y	Total
Y	2	40	42
N	6	2	8
Total	8	42	50

16-19 Derivative words

	Derivative user		
Specific instrument words	N	Y	Total
Y	0	46	46
N	1	3	4
Total	1	49	50

20-30 Derivative words

	Derivative user		
Specific instrument words	N	Y	Total
Y	0	45	45
N	0	5	5
Total	0	50	50

All categories examined

	Derivative user		
Specific instrument words	N	Y	Total
Y	8	149	157
N	67	26	93
Total	75	175	250

This table reports the two-way frequency table of derivative users and non-users and observations with and without specific instrument words for random subsamples of the SeekEdgar sample (table C.1, panel A), by number of derivative words. Each subsample includes 50 random observations for the latest pre-period year, 1998 or 1999, totalling 250 random observations.

Appendix D

Alternate Specifications of Model for H1

This appendix reports the results of additional analyses of H1, discussed in section 4.2.3.6.

TABLE D.1

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts – Full Set of Control Variables (H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample
TREAT	?	0.003
		[0.18]
POST	?	0.037 **
		[2.06]
TREAT × POST	?	-0.008
		[-0.40]
MF_SURP	+	6.867 ***
		[2.72]
TREAT × MF_SURP	?	-1.770
		[-1.02]
POST × MF_SURP	+	1.900
		[0.93]
TREAT × POST × MF_SURP	-	0.929
		[0.42]
HiOCFVOL	?	-0.017 **
		[-2.27]
HiOCFVOL × MF_SURP	-	-2.642 **
		[-1.75]
MF_SURP × |MF_SURP|	-	-43.966 **
		[-1.75]
MF_LOSS	?	-0.010
		[-0.28]
MF_LOSS × MF_SURP	-	-1.524
		[-1.03]
HiMF_WIDTH	?	-0.016 **
		[-2.00]
HiMF_WIDTH × MF_SURP	-	1.693 *
		[1.39]
HiMF_HORIZON	?	0.006
		[0.92]
HiMF_HORIZON × MF_SURP	-	0.231
		[0.30]
HiMTB	?	-0.016 **
		[-2.38]
HiMTB × MF_SURP	-	0.168
		[0.17]

Continued on next page

TABLE D.1 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample
HiMVE	?	-0.005
		[-0.69]
HiMVE × *MF_SURP*	-	-1.160
		[-1.07]
EA_CONCUR	?	0.006
		[0.85]
EA_CONCUR × *MF_SURP*	?	-2.161 **
		[-2.26]
EA_SURP	+	0.117
		[0.24]
EA_SURP × |*EA_SURP*|	-	0.212
		[0.17]
EA_LOSS	?	0.002
		[0.10]
EA_LOSS × *EA_SURP*	-	0.046
		[0.15]
HiMTB × *EA_SURP*	-	-0.283
		[-1.13]
HiMVE × *EA_SURP*	-	0.081
		[0.32]
Constant		-0.008
		[-0.52]
N		667
Adjusted R^2		0.190
df_m		28
df_r		230

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TABLE D.1 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*

Variable	Predicted Sign	DV: *MF_CAR* $_{0,1}$ Constant Derivative Unmatched Sample	
TREAT	?	-0.012	
		[-0.79]	
POST	?	0.023	
		[1.15]	
TREAT × POST	?	-0.001	
		[-0.06]	
MF_SURP_GNEWS	+	0.419	
		[0.09]	
MF_SURP_BNEWS	+	9.742	***
		[2.82]	
TREAT × MF_SURP_GNEWS	?	10.106	**
		[2.15]	
TREAT × MF_SURP_BNEWS	?	-4.254	***
		[-2.65]	
POST × MF_SURP_GNEWS	+	17.078	**
		[2.07]	
POST × MF_SURP_BNEWS	+	-0.036	
		[-0.02]	
TREAT × POST × MF_SURP_GNEWS	-	-16.401	**
		[-1.99]	
TREAT × POST × MF_SURP_BNEWS	-	1.319	
		[0.57]	
HiOCFVOL	?	-0.009	
		[-1.15]	
HiOCFVOL × MF_SURP_GNEWS	-	-6.832	**
		[-2.07]	
HiOCFVOL × MF_SURP_BNEWS	-	-1.067	
		[-0.77]	
MF_SURP_GNEWS × |MF_SURP|	-	-23.129	
		[-0.33]	
MF_SURP_BNEWS × |MF_SURP|	-	-79.223	***
		[-2.41]	
MF_LOSS	?	0.079	***
		[2.66]	
MF_LOSS × MF_SURP_GNEWS	-	-22.042	***
		[-3.48]	

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TABLE D.1 - *Continued*

		DV: *MF_CAR* $_{0,1}$
	Predicted	Constant Derivative
Variable	Sign	Unmatched Sample
MF_LOSS × *MF_SURP_BNEWS*	-	0.510
		[0.28]
HiMF_WIDTH	?	-0.010
		[-1.63]
HiMF_WIDTH × *MF_SURP_GNEWS*	-	3.291 *
		[1.30]
HiMF_WIDTH × *MF_SURP_BNEWS*	-	1.587
		[1.22]
HiMF_HORIZON	?	0.003
		[0.42]
HiMF_HORIZON × *MF_SURP_GNEWS*	-	-1.841
		[-0.79]
HiMF_HORIZON × *MF_SURP_BNEWS*	-	0.190
		[0.19]
HiMTB	?	-0.016 **
		[-2.05]
HiMTB × *MF_SURP_GNEWS*	-	-4.047 *
		[-1.53]
HiMTB × *MF_SURP_BNEWS*	-	-0.578
		[-0.38]
HiMVE	?	0.016 **
		[2.02]
HiMVE × *MF_SURP_GNEWS*	-	-6.192 ***
	-	[-2.57]
HiMVE × *MF_SURP_BNEWS*	?	0.780
		[0.61]
EA_CONCUR	?	-0.006
		[-0.82]
EA_CONCUR × *MF_SURP_GNEWS*	?	3.841
		[1.15]
EA_CONCUR × *MF_SURP_BNEWS*	?	-3.617 ***
		[-3.63]
EA_SURP_GNEWS	+	0.009
		[0.02]
EA_SURP_BNEWS	+	-1.374 *
		[-1.38]
EA_SURP_GNEWS × |*EA_SURP*|	-	0.779
		[0.56]

Continued on next page

TABLE D.1 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample
$EA\_SURP\_BNEWS \times \|EA\_SURP\|$	-	-0.574
		[-0.28]
EA_LOSS	?	0.033
		[1.54]
$EA\_LOSS \times EA\_SURP\_GNEWS$	-	-0.037
		[-0.11]
$EA\_LOSS \times EA\_SURP\_BNEWS$	-	2.193 **
		[2.28]
$HiMTB \times EA\_SURP\_GNEWS$	-	0.006
		[0.02]
$HiMTB \times EA\_SURP\_BNEWS$	-	-1.700 *
		[-1.71]
$HiMVE \times EA\_SURP\_GNEWS$	-	-0.130
		[-0.48]
$HiMVE \times EA\_SURP\_BNEWS$	-	2.207 **
		[2.51]
Constant	?	-0.003
		[-0.17]
N		667
Adjusted R^2		0.258
df_m		45
df_r		230

This table reports the results of the analysis of H1, including the full set of control variables considered and discussed in section 3.2.2, using the constant derivative unmatched sample (table 4.4, panel A). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE D.2

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Industry Fixed Effects (H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$	
		Constant Derivative Unmatched Sample	
Variable	Predicted Sign	(1)	(2)
TREAT	?	-0.002	0.000
		[-0.11]	[-0.03]
POST	?	0.043 **	0.046 ***
		[2.43]	[2.63]
TREAT × POST	?	-0.012	-0.014
		[-0.66]	[-0.75]
MF_SURP	+	4.973 ***	6.298 **
		[2.48]	[2.14]
TREAT × MF_SURP	?	-0.601	2.300
		[-0.40]	[1.15]
POST × MF_SURP	+	2.165	4.321 **
		[1.25]	[2.24]
TREAT × POST × MF_SURP	-	-0.530	-3.278 *
		[-0.24]	[-1.36]
HiOCFVOL	?	-0.017 **	-0.017 **
		[-2.22]	[-2.27]
HiOCFVOL × MF_SURP	-	-2.766 **	-3.214 **
		[-1.84]	[-1.78]
MF_SURP × |MF_SURP|	-	-31.148	-16.059
		[-1.06]	[-0.62]
MF_LOSS	?	0.009	0.007
		[0.29]	[0.28]
MF_LOSS × MF_SURP	-	-1.401	-3.019 **
		[-0.84]	[-1.72]
HiMF_WIDTH	?	-0.012	-0.016 **
		[-1.50]	[-1.99]
HiMF_WIDTH × MF_SURP	-	2.072 **	0.621
		[1.95]	[0.40]
HiMVE	?	-0.010	-0.012
		[-1.28]	[-1.59]
HiMVE × MF_SURP	-	-1.286	-2.610 ***
		[-1.16]	[-2.47]
Constant	?	0.006	0.010
		[0.30]	[0.50]
Industry fixed effects		Yes	No
Industry fixed effects interacted with *MF_SURP*		No	Yes
N		667	667
Adjusted R^2		0.182	0.203
df_m		26	36
df_r		230	230

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TABLE D.2 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*					
		DV: $MF\_CAR_{0,1}$			
	Predicted	Constant Derivative Unmatched Sample			
Variable	Sign	(1)		(2)	
TREAT	?	-0.013		-0.005	
		[-0.69]		[-0.30]	
POST	?	0.024		0.029	
		[1.26]		[1.49]	
TREAT × *POST*	?	-0.007		-0.015	
		[-0.33]		[-0.69]	
MF_SURP_GNEWS	+	-0.569		-0.570	
		[-0.17]		[-0.16]	
MF_SURP_BNEWS	+	6.531	***	4.584	**
		[2.71]		[1.80]	
TREAT × *MF_SURP_GNEWS*	?	8.411	**	9.214	**
		[2.19]		[2.26]	
TREAT × *MF_SURP_BNEWS*	?	-2.211		1.989	
		[-1.43]		[0.94]	
POST × *MF_SURP_GNEWS*	+	21.705	***	21.544	***
		[3.06]		[2.81]	
POST × *MF_SURP_BNEWS*	+	0.899		3.474	*
		[0.49]		[1.56]	
TREAT × *POST* × *MF_SURP_GNEWS*	-	-16.841	***	-16.184	**
		[-2.51]		[-2.22]	
TREAT × *POST* × *MF_SURP_BNEWS*	-	-0.707		-4.557	*
		[-0.31]		[-1.47]	
HiOCFVOL	?	-0.011		-0.010	
		[-1.45]		[-1.36]	
HiOCFVOL × *MF_SURP_GNEWS*	-	-4.837	**	-6.099	**
		[-1.92]		[-2.13]	
HiOCFVOL × *MF_SURP_BNEWS*	-	-0.792		-0.870	
		[-0.50]		[-0.66]	
MF_SURP_GNEWS × |*MF_SURP*|	-	64.143		66.317	
		[0.81]		[0.76]	
MF_SURP_BNEWS × |*MF_SURP*|	-	-42.049	*	-25.948	
		[-1.43]		[-0.98]	
MF_LOSS	?	0.098	***	0.081	***
		[3.30]		[2.85]	
MF_LOSS × *MF_SURP_GNEWS*	-	-23.516	***	-22.196	***
		[-4.12]		[-3.62]	
MF_LOSS × *MF_SURP_BNEWS*	-	0.458		-2.455	
		[0.26]		[-1.15]	

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TABLE D.2 - *Continued*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)	(2)
HiMVE	?	0.008	0.007
		[1.11]	[1.03]
HiMVE × *MF_SURP_GNEWS*	-	-8.341 ***	-9.680 ***
		[-2.53]	[-3.42]
HiMVE × *MF_SURP_BNEWS*	-	0.500	0.171
		[0.36]	[0.13]
Constant	?	-0.001	-0.002
		[-0.03]	[-0.09]
Industry fixed effects		Yes	No
Industry fixed effects interacted with *MF_SURP_GNEWS* and *MF_SURP_BNEWS*		No	Yes
N		667	667
Adjusted R^2		0.22	0.244
df_m		32	42
df_r		230	230

This table reports the results of the analysis of H1 using the constant derivative unmatched sample (table 4.4, panel A), after including industry fixed effects. Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE D.3

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts – Continuous Control Variables (H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*

		DV: *MF_CAR* $_{0,1}$	
		Constant Derivative Unmatched Sample	Constant Derivative Matched Sample
Variable	Predicted Sign	(1)	(2)
TREAT	?	0.002	-0.018
		[0.12]	[-0.77]
POST	?	0.031 *	0.045 *
		[1.78]	[1.93]
TREAT × *POST*	?	-0.008	0.003
		[-0.41]	[0.10]
MF_SURP	+	9.020 ***	3.725 ***
		[3.15]	[2.45]
TREAT × *MF_SURP*	?	-0.667	-0.499
		[-0.38]	[-0.39]
POST × *MF_SURP*	+	1.473	3.272 ***
		[0.76]	[3.58]
TREAT × *POST* × *MF_SURP*	-	0.357	-1.597
		[0.15]	[-0.74]
OCFVOL	?	-0.142	
		[-1.22]	
OCFVOL × *MF_SURP*	-	-4.577	
		[-0.30]	
MF_SURP × |*MF_SURP*|	-	-31.885	-35.027 *
		[-1.10]	[-1.52]
MF_LOSS	?	0.015	
		[0.36]	
MF_LOSS × *MF_SURP*	-	-1.542	
		[-0.72]	
MF_WIDTH	?	0.298	
		[0.15]	
MF_WIDTH × *MF_SURP*	-	-35.664	
		[-0.53]	
MVE	?	-0.003	
		[-1.55]	
MVE × *MF_SURP*	-	-0.572 **	
		[-1.88]	
Constant	?	0.005	-0.030
		[0.25]	[-1.59]
N		667	219
Adjusted R^2		0.163	0.163
df_m		16	8
df_r		230	78

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TABLE D.3 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$	
		Constant Derivative Unmatched Sample	Constant Derivative Matched Sample
Variable	Predicted Sign	(1)	(2)
TREAT	?	-0.011	-0.038
		[-0.69]	[-1.28]
POST	?	0.020	0.023
		[1.01]	[0.84]
TREAT × *POST*	?	-0.001	0.039
		[-0.07]	[1.13]
MF_SURP_GNEWS	+	9.674 **	6.618 **
		[2.14]	[2.34]
MF_SURP_BNEWS	+	4.962 *	-7.233
		[1.40]	[-0.37]
TREAT × *MF_SURP_GNEWS*	?	7.607 *	7.988 ***
		[1.86]	[2.89]
TREAT × *MF_SURP_BNEWS*	?	-2.321	0.701
		[-1.30]	[0.10]
POST × *MF_SURP_GNEWS*	+	17.720 ***	14.021 *
		[2.68]	[1.62]
POST × *MF_SURP_BNEWS*	+	1.298	2.796
		[0.63]	[0.45]
TREAT × *POST* × *MF_SURP_GNEWS*	-	-15.032 **	-18.536 **
		[-2.27]	[-2.18]
TREAT × *POST* × *MF_SURP_BNEWS*	-	-1.026	0.550
		[-0.41]	[0.08]
OCFVOL	?	-0.150	-0.120
		[-1.21]	[-0.47]
OCFVOL × *MF_SURP_GNEWS*	-	-71.638 **	-87.822 *
		[-1.74]	[-1.44]
OCFVOL × *MF_SURP_BNEWS*	-	2.780	51.710
		[0.16]	[0.68]
MF_SURP_GNEWS × |*MF_SURP*|	-	3.344	
		[0.06]	
MF_SURP_BNEWS × |*MF_SURP*|	-	-54.818 **	
		[-2.10]	
MF_LOSS	?	0.128 ***	
		[2.73]	
MF_LOSS × *MF_SURP_GNEWS*	-	-23.546 ***	
		[-4.07]	
MF_LOSS × *MF_SURP_BNEWS*	-	0.985	
		[0.42]	

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TABLE D.3 - *Continued*

		DV: $MF\_CAR_{0,1}$	
	Predicted	Constant Derivative Unmatched Sample	Constant Derivative Matched Sample
Variable	Sign	(1)	(2)
MVE	?	0.000	0.002
		[0.09]	[0.48]
MVE × MF_SURP_GNEWS	-	-1.464 **	-0.812 **
		[-2.27]	[-2.31]
MVE × MF_SURP_BNEWS	-	0.234	0.753
		[0.64]	[0.56]
EA_CONCUR	?		-0.007
			[-0.48]
EA_CONCUR × MF_SURP_GNEWS	?		4.501 **
			[2.02]
EA_CONCUR × MF_SURP_BNEWS	?		-0.881
			[-0.22]
Constant		-0.008	-0.033
		[-0.34]	[-0.70]
N		667	219
Adjusted R^2		0.206	0.163
r2		0.232	0.24
df_m		22	20
df_r		230	78

This table reports the results of the analysis of H1 using the constant derivative unmatched and matched samples (table 4.4), using continuous control variables instead of binary variables. Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE D.4

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Exclude Stand-Alone Control Variables (H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*

		DV: $MF\_CAR_{0,1}$	
		Constant Derivative	Constant Derivative
	Predicted	Unmatched Sample	Matched Sample
Variable	Sign	(1)	(2)
TREAT	?	0.006	-0.018
		[0.39]	[-0.77]
POST	?	0.041 **	0.045 *
		[2.35]	[1.93]
TREAT × *POST*	?	-0.015	0.003
		[-0.78]	[0.10]
MF_SURP	+	5.325 ***	3.725 ***
		[2.69]	[2.45]
TREAT × *MF_SURP*	?	-1.015	-0.499
		[-0.73]	[-0.39]
POST × *MF_SURP*	+	1.856	3.272 ***
		[1.10]	[3.58]
TREAT × *POST* × *MF_SURP*	-	-0.281	-1.597
		[-0.12]	[-0.74]
HiOCFVOL × *MF_SURP*	-	-2.423 *	
		[-1.56]	
MF_SURP × |*MF_SURP*|	-	-40.073 *	-35.027 *
		[-1.34]	[-1.52]
MF_LOSS × *MF_SURP*	-	-1.173	
		[-0.82]	
HiMF_WIDTH × *MF_SURP*	-	2.262 **	
		[2.16]	
HiMVE × *MF_SURP*	-	-1.258	
		[-1.09]	
Constant	?	-0.03 **	-0.03
		[-2.28]	[-1.59]
N		667	219
Adjusted R^2		0.174	0.163
df_m		12	8
df_r		230	78

Continued on next page

TABLE D.4 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*

Variable	Predicted Sign	DV: $MF\_CAR_{0,1}$ Constant Derivative Unmatched Sample (1)	DV: $MF\_CAR_{0,1}$ Constant Derivative Matched Sample (2)
TREAT	?	-0.003	-0.036
		[-0.17]	[-1.28]
POST	?	0.028	0.024
		[1.51]	[0.87]
TREAT × *POST*	?	-0.012	0.034
		[-0.57]	[1.00]
MF_SURP_GNEWS	+	0.510	0.330
		[0.14]	[0.18]
MF_SURP_BNEWS	+	6.216 ***	0.348
		[2.64]	[0.07]
TREAT × *MF_SURP_GNEWS*	?	7.269 *	9.385 ***
		[1.84]	[2.98]
TREAT × *MF_SURP_BNEWS*	?	-2.256	-1.053
		[-1.49]	[-0.26]
POST × *MF_SURP_GNEWS*	+	17.312 ***	13.523 *
		[2.40]	[1.55]
POST × *MF_SURP_BNEWS*	+	1.452	0.372
		[0.79]	[0.20]
TREAT × *POST* × *MF_SURP_GNEWS*	-	-12.398 **	-16.000 **
		[-1.82]	[-1.96]
TREAT × *POST* × *MF_SURP_BNEWS*	-	-1.154	2.486
		[-0.50]	[0.62]
HiOCFVOL × *MF_SURP_GNEWS*	-	-5.247 **	-3.941 *
		[-2.14]	[-1.44]
HiOCFVOL × *MF_SURP_BNEWS*	-	-0.245	3.435
		[-0.16]	[1.16]
MF_SURP_GNEWS × |*MF_SURP*|	-	65.713	
		[0.83]	
MF_SURP_BNEWS × |*MF_SURP*|	-	-40.917 *	
		[-1.40]	
MF_LOSS × *MF_SURP_GNEWS*	-	-17.075 ***	
		[-3.19]	
MF_LOSS × *MF_SURP_BNEWS*	-	-1.434	
		[-0.88]	
HiMVE × *MF_SURP_GNEWS*	-	-8.249 ***	-5.058 ***
		[-2.63]	[-3.23]
HiMVE × *MF_SURP_BNEWS*	-	0.484	1.380
		[0.35]	[0.41]

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TABLE D.4 - *Continued*

		DV: $MF\_CAR_{0,1}$	
		Constant Derivative	Constant Derivative
	Predicted	Unmatched Sample	Matched Sample
Variable	Sign	(1)	(2)
EA_CONCUR × MF_SURP_GNEWS	*?*		3.357
			[1.27]
EA_CONCUR × MF_SURP_BNEWS	*?*		-1.750
			[-0.90]
Constant	*?*	-0.020	-0.025
		[-1.37]	[-1.17]
N		667	219
Adjusted R^2		0.215	0.171
df_m		19	17
df_r		230	78

This table reports the results of the analysis of H1 using the constant samples (table 4.4), excluding the stand-alone control variables. Only the *MF_SURP-* (*MF_SURP_GNEWS-* and *MF_SURP_BNEWS-)* interacted control variables are included. Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panel B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE D.5

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - One Control at a Time (H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*, Unmatched Samples

	DV: *MF_CAR* $_{0,1}$									
	Full Unmatched Sample					Constant Derivative Unmatched Sample				
Variable	HiOCFVOL	|MF_SURP|	MF_LOSS	HiMF_WIDTH	HiMVE	HiOCFVOL	|MF_SURP|	MF_LOSS	HiMF_WIDTH	HiMVE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TREAT	-0.009	-0.007	-0.007	-0.007	-0.004	-0.001	0.004	0.002	0.002	0.007
	[-0.95]	[-0.70]	[-0.68]	[-0.73]	[-0.42]	[-0.05]	[0.26]	[0.13]	[0.14]	[0.46]
POST	0.032 ***	0.030 ***	0.031 ***	0.034 ***	0.031 ***	0.037 **	0.037 **	0.039 **	0.039 **	0.036 **
	[2.92]	[2.76]	[2.91]	[3.12]	[2.85]	[2.10]	[2.16]	[2.32]	[2.28]	[2.07]
TREAT × POST	-0.002	-0.002	-0.002	-0.002	-0.002	-0.008	-0.011	-0.011	-0.008	-0.009
	[-0.12]	[-0.17]	[-0.16]	[-0.16]	[-0.19]	[-0.43]	[-0.57]	[-0.61]	[-0.40]	[-0.48]
MF_SURP	2.647 **	4.355 ***	1.778 **	0.883	1.807 **	5.592 ***	5.972 ***	3.421 ***	2.742 **	3.427 ***
	[1.76]	[3.56]	[1.85]	[0.98]	[1.88]	[3.08]	[4.05]	[2.61]	[1.66]	[2.62]
TREAT × MF_SURP	-0.484	-0.452	-0.136	-0.287	0.459	-2.502 *	-1.909	-1.521	-1.649	-1.158
	[-0.40]	[-0.51]	[-0.12]	[-0.29]	[0.34]	[-1.71]	[-1.59]	[-1.02]	[-1.09]	[-0.71]
POST × MF_SURP	0.593	1.203	1.021	0.438	0.700	0.065	0.882	2.272 *	-0.154	-0.245
	[0.60]	[1.28]	[1.15]	[0.51]	[0.74]	[0.04]	[0.50]	[1.34]	[-0.10]	[-0.16]
TREAT × POST × MF_SURP	2.604 *	1.081	2.082 *	2.636 **	2.325 *	2.904 *	1.231	0.463	3.232 *	3.172 *
	[1.60]	[0.66]	[1.38]	[1.72]	[1.53]	[1.45]	[0.51]	[0.21]	[1.61]	[1.61]
[Var]	-0.016 **		0.006	-0.012 *	-0.005	-0.017 **		-0.001	-0.014 *	-0.008
	[-2.51]		[0.27]	[-1.71]	[-0.73]	[-2.29]		[-0.03]	[-1.78]	[-0.99]
[Var] × MF_SURP	-1.070	-48.331 ***	-0.718	1.300 **	-1.323	-2.602 ***	-52.013 **	-3.085 ***	0.498	-1.397
	[-1.07]	[-2.64]	[-0.62]	[1.74]	[-1.24]	[-1.98]	[-2.42]	[-3.35]	[0.46]	[-1.23]
Constant	-0.012	-0.019 **	-0.021 **	-0.016 **	-0.019 **	-0.019	-0.028 **	-0.028 **	-0.024 *	-0.027 **
	[-1.32]	[-2.25]	[-2.44]	[-2.03]	[-2.28]	[-1.36]	[-2.19]	[-2.17]	[-1.93]	[-2.12]
N	828	828	828	828	828	667	667	667	667	667
adj. R-sq	0.138	0.149	0.131	0.14	0.134	0.158	0.157	0.151	0.144	0.143
df_m	9	8	9	9	9	9	8	9	9	9
df_r	283	283	283	283	283	230	230	230	230	230

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TABLE D.5 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*, Unmatched Samples

	DV: $MF\_CAR_{0,1}$							
	Full Unmatched Sample				Constant Derivative Unmatched Sample			
Variable	HiOCFVOL (1)	|MF_SURP| (2)	MF_LOSS (3)	HiMVE (4)	HiOCFVOL (5)	|MF_SURP| (6)	MF_LOSS (7)	HiMVE (8)
TREAT	-0.009	-0.008	-0.004	-0.013	-0.005	-0.001	0.003	-0.008
	[-0.74]	[-0.73]	[-0.36]	[-1.05]	[-0.31]	[-0.08]	[0.19]	[-0.52]
POST	0.029 **	0.034 ***	0.024 *	0.032 **	0.033 *	0.042 **	0.028	0.037 **
	[2.29]	[2.81]	[1.90]	[2.54]	[1.86]	[2.43]	[1.50]	[2.09]
TREAT × POST	-0.009	-0.016	-0.004	-0.011	-0.016	-0.025	-0.010	-0.017
	[-0.62]	[-1.08]	[-0.28]	[-0.72]	[-0.82]	[-1.33]	[-0.50]	[-0.86]
MF_SURP_GNEWS	3.389	5.113 *	3.126	3.332	1.405	3.614 *	1.828	1.591
	[0.83]	[1.63]	[0.85]	[0.89]	[0.51]	[1.51]	[0.68]	[0.64]
MF_SURP_BNEWS	1.798	4.242 ***	1.680 *	1.621 *	5.582 ***	6.668 ***	3.635 **	3.610 ***
	[1.13]	[3.00]	[1.64]	[1.59]	[2.74]	[4.21]	[2.42]	[2.41]
TREAT × MF_SURP_GNEWS	0.203	2.048	-1.745	5.292	2.333	2.775	-0.512	7.487 *
	[0.05]	[0.58]	[-0.46]	[1.14]	[0.68]	[1.00]	[-0.18]	[1.95]
TREAT × MF_SURP_BNEWS	-0.148	-0.393	-0.005	-0.442	-2.684	-2.252 *	-1.583	-2.498
	[-0.10]	[-0.37]	[-0.00]	[-0.32]	[-1.57]	[-1.67]	[-0.89]	[-1.41]
POST × MF_SURP_GNEWS	3.825	-0.669	5.606	0.048	5.128	-0.590	14.378 **	-0.346
	[0.70]	[-0.17]	[1.23]	[0.01]	[0.95]	[-0.16]	[2.30]	[-0.07]
POST × MF_SURP_BNEWS	0.695	1.315 *	0.704	0.771	-0.413	0.918	1.443	-0.311
	[0.66]	[1.49]	[0.75]	[0.82]	[-0.26]	[0.59]	[0.81]	[-0.20]
TREAT × POST × MF_SURP_GNEWS	3.574	5.162	0.698	3.165	2.109	5.178	-8.260	3.248
	[0.60]	[0.99]	[0.12]	[0.57]	[0.37]	[1.01]	[-1.16]	[0.65]
TREAT × POST × MF_SURP_BNEWS	0.653	-0.913	0.668	0.621	1.417	-0.730	-0.409	1.729
	[0.42]	[-0.64]	[0.45]	[0.40]	[0.71]	[-0.34]	[-0.18]	[0.87]
[Var]	-0.009		0.051 ***	0.013 *	-0.012 *		0.062 **	0.011
	[-1.39]		[2.77]	[1.66]	[-1.65]		[2.50]	[1.37]
[Var] × MF_SURP_GNEWS	-4.881 **	-126.198 ***	-13.336 ***	-8.216 ***	-5.342 **	-110.819 **	-20.860 ***	-8.743 ***
	[-1.78]	[-2.59]	[-5.19]	[-2.67]	[-1.83]	[-2.13]	[-3.84]	[-2.69]
[Var] × MF_SURP_BNEWS	-0.144	-45.671 **	0.365	0.635	-1.953 *	-56.419 ***	-1.641 **	0.697
	[-0.15]	[-2.21]	[0.31]	[0.48]	[-1.36]	[-2.52]	[-1.75]	[0.49]
Constant	-0.015	-0.019 **	-0.023 **	-0.025 **	-0.015	-0.023 *	-0.028 *	-0.028 *
	[-1.53]	[-1.97]	[-2.37]	[-2.57]	[-1.04]	[-1.68]	[-1.94]	[-1.95]

Continued on next page

TABLE D.5 - *Continued*

	DV: $MF\_CAR_{0,1}$							
	Full Unmatched Sample				Constant Derivative Unmatched Sample			
Variable	HiOCFVOL	|MF_SURP|	MF_LOSS	HiMVE	HiOCFVOL	|MF_SURP|	MF_LOSS	HiMVE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
N	828	828	828	828	667	667	667	667
adj. R-sq	0.156	0.161	0.147	0.162	0.176	0.172	0.168	0.178
df_m	14	13	14	14	14	13	14	14
df_r	283	283	283	283	230	230	230	230

This table reports the results of the analysis of H1 using the full unmatched sample (table 3.1, panel B) and the constant derivative unmatched sample (table 4.4, panel A), including one control variable, and its interaction(s) with *MF_SURP* (*MF_SURP_GNEWS* and *MF_SURP_BNEWS*), at a time. Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panels B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the two-day cumulative abnormal return around the management forecast, $MF\_CAR_{0,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

TABLE D.6

The Effect of Exposure to Fair Value Accounting on the Credibility of Management Forecasts - Three-day MF_CAR (H1)

Panel A: Management Forecast Response Coefficient, Not Conditioned on Sign of *MF_SURP*

		DV: $MF\_CAR_{-1,1}$	
		Constant Derivative Unmatched Sample	Constant Derivative Matched Sample
Variable	Predicted Sign	(1)	(2)
TREAT	?	0.004	-0.025
		[0.23]	[-1.03]
POST	?	0.047 **	0.044 *
		[2.55]	[1.80]
TREAT × POST	?	-0.017	0.002
		[-0.85]	[0.06]
MF_SURP	+	5.618 **	1.645
		[2.17]	[1.00]
TREAT × MF_SURP	?	-0.806	0.741
		[-0.41]	[0.51]
POST × MF_SURP	+	2.542	5.203 ***
		[1.11]	[3.95]
TREAT × POST × MF_SURP	-	-1.124	-4.360 **
		[-0.42]	[-1.98]
HiOCFVOL	?	-0.012	
		[-1.56]	
HiOCFVOL × MF_SURP	-	-3.910 **	
		[-2.23]	
MF_SURP × |MF_SURP|	-	-24.580	-1.501
		[-0.60]	[-0.05]
MF_LOSS	?	0.012	
		[0.31]	
MF_LOSS × MF_SURP	-	-0.644	
		[-0.29]	
HiMF_WIDTH	?	-0.008	
		[-0.99]	
HiMF_WIDTH × MF_SURP	-	2.378 **	
		[1.94]	
HiMVE	?	-0.007	
		[-0.88]	
HiMVE × MF_SURP	-	-1.514	
		[-1.18]	
Constant		-0.019	-0.024
		[-1.17]	[-1.15]
N		667	219
Adjusted R^2		0.186	0.222
df_m		16	8
df_r		230	78

Continued on next page

TABLE D.6 - *Continued*

Panel B: Management Forecast Response Coefficient, Conditioned on Sign of *MF_SURP*

Variable	Predicted Sign	DV: MF_CAR$_{-1,1}$ Constant Derivative Unmatched Sample (1)	DV: MF_CAR$_{-1,1}$ Constant Derivative Matched Sample (2)
TREAT	?	-0.008	-0.042
		[-0.44]	[-1.35]
POST	?	0.028	0.022
		[1.56]	[0.84]
TREAT × POST	?	-0.010	0.036
		[-0.53]	[1.06]
MF_SURP_GNEWS	+	1.293	-0.147
		[0.34]	[-0.08]
MF_SURP_BNEWS	+	7.456 **	2.384
		[2.27]	[0.48]
TREAT × MF_SURP_GNEWS	?	7.943 *	9.474 **
		[1.81]	[2.14]
TREAT × MF_SURP_BNEWS	?	-2.483	-3.171
		[-1.20]	[-0.78]
POST × MF_SURP_GNEWS	+	22.590 ***	17.156 **
		[2.72]	[1.72]
POST × MF_SURP_BNEWS	+	1.122	1.125
		[0.46]	[0.57]
TREAT × POST × MF_SURP_GNEWS	-	-18.126 **	-22.740 **
		[-2.29]	[-2.30]
TREAT × POST × MF_SURP_BNEWS	-	-1.180	1.870
		[-0.44]	[0.46]
HiOCFVOL	?	-0.009	0.000
		[-1.15]	[0.01]
HiOCFVOL × MF_SURP_GNEWS	-	-5.042 **	-3.084
		[-1.82]	[-0.75]
HiOCFVOL × MF_SURP_BNEWS	-	-2.330	3.200
		[-1.17]	[0.97]
MF_SURP_GNEWS × |MF_SURP|	-	36.791	
		[0.43]	
MF_SURP_BNEWS × |MF_SURP|	-	-29.195	
		[-0.67]	
MF_LOSS	?	0.114 ***	
		[2.60]	
MF_LOSS × MF_SURP_GNEWS	-	-25.924 ***	
		[-4.27]	
MF_LOSS × MF_SURP_BNEWS	-	1.464	
		[0.57]	
HiMVE	?	0.011	0.014
		[1.22]	[0.88]

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TABLE D.6 - *Continued*

		DV: $MF\_CAR_{-1,1}$	
	Predicted	Constant Derivative Unmatched Sample	Constant Derivative Matched Sample
Variable	Sign	(1)	(2)
HiMVE × MF_SURP_GNEWS	-	-8.169 **	-5.661 ***
		[-2.18]	[-4.18]
HiMVE × MF_SURP_BNEWS	-	0.201	4.796
		[0.12]	[1.24]
EA_CONCUR	?		-0.005
			[-0.34]
EA_CONCUR × MF_SURP_GNEWS	?		5.900 ***
			[2.67]
EA_CONCUR × MF_SURP_BNEWS	?		-3.823 *
			[-1.91]
Constant	?	-0.021	-0.022
		[-1.23]	[-0.87]
N		667	219
Adjusted R^2		0.22	0.234
df_m		22	20
df_r		230	78

This table reports the results of the additional analysis of H1, using the constant derivative samples (table 4.4). Panel A reports the regression results, where the management forecast response coefficient is not conditioned on the sign of the management forecast surprise (*MF_SURP*). Panels B reports the regression results, where the management forecast response coefficient is conditioned on the sign of *MF_SURP*. The dependent variable is the *three*-day cumulative abnormal return around the management forecast, $MF\_CAR_{-1,1}$. *TREAT* is an indicator variable that equals one for firms that use derivatives in the latest pre-period, and zero otherwise. *POST* is an indicator variable that equals one for post-SFAS 133 periods, and zero otherwise. ***, **, and * indicate statistical significance at the 1%, 5%, 10% levels, respectively, based on the one-tailed tests for signed predictions and the two-tailed tests otherwise. All t-statistics, in brackets, are estimated using firm-level clustering and heteroscedasticity-robust standard errors (Petersen 2009). All variables are defined in Appendix A.

Appendix E

Null Distributions of *ΔIPT* and *DID_IPT*

In this appendix, I present the null distributions of the portfolio-level *ΔIPT* and *DID_IPT,* corresponding to the tests of H2 in section 4.3. Table E.1 presents the null distributions for the test of H2 using the full samples in table 4.18. Table E.2 presents the null distributions for the test of H2 using the constant derivative samples in table 4.20. Finally, table E.3 presents the null distributions for the test of H2 using the alternative matched samples in table 4.21.

TABLE E.1
Null Distribution of IPT (H2)

Panel A: Unmatched Sample

Sample/Subsample	Statistic	N	mean	sd	min	p1	p5	p10	p25	p50	p75	p90	p95	p99	max
Combined	*ΔIPT(control)*	1000	0.414	1.002	-2.299	-1.801	-1.304	-0.952	-0.320	0.472	1.143	1.719	2.021	2.423	3.167
	ΔIPT(treat)	1000	0.462	0.775	-1.662	-1.216	-0.767	-0.563	-0.099	0.467	0.989	1.488	1.743	2.180	3.072
	DiD_IPT	1000	0.048	0.540	-1.467	-1.108	-0.793	-0.637	-0.348	0.025	0.420	0.789	0.950	1.312	1.517
Positive intraperiod return	*ΔIPT(control)*	1000	0.681	1.197	-2.535	-1.899	-1.330	-0.888	-0.214	0.737	1.532	2.239	2.601	3.287	4.151
	ΔIPT(treat)	1000	0.704	1.045	-1.767	-1.272	-0.932	-0.638	-0.060	0.640	1.431	2.075	2.523	3.154	3.781
	DiD_IPT	1000	0.023	1.153	-2.936	-2.344	-1.898	-1.431	-0.833	0.004	0.794	1.581	2.008	2.618	3.466
Negative intraperiod return	*ΔIPT(control)*	1000	0.367	1.112	-2.512	-1.966	-1.430	-1.080	-0.489	0.382	1.263	1.838	2.129	2.494	2.867
	ΔIPT(treat)	1000	0.301	1.611	-3.612	-2.968	-2.299	-1.823	-0.968	0.327	1.532	2.445	2.857	3.697	4.211
	DiD_IPT	1000	-0.066	0.811	-2.076	-1.781	-1.346	-1.151	-0.665	-0.054	0.532	0.987	1.271	1.726	2.191

Panel B: Matched Sample

Sample/Subsample	Statistic	N	mean	sd	min	p1	p5	p10	p25	p50	p75	p90	p95	p99	max
Combined	*ΔIPT(control)*	1000	0.439	1.261	-2.739	-2.323	-1.726	-1.219	-0.449	0.436	1.360	2.081	2.535	3.289	3.967
	ΔIPT(treat)	1000	0.454	1.016	-2.054	-1.760	-1.220	-0.908	-0.303	0.448	1.195	1.775	2.095	2.750	3.231
	DiD_IPT	1000	0.015	0.679	-2.010	-1.579	-1.137	-0.882	-0.454	0.034	0.508	0.908	1.093	1.411	1.835
Positive intraperiod return	*ΔIPT(control)*	1000	0.650	1.372	-3.123	-2.108	-1.458	-1.077	-0.384	0.584	1.643	2.497	2.943	3.802	4.812
	ΔIPT(treat)	1000	0.650	1.473	-3.023	-2.491	-1.810	-1.285	-0.403	0.647	1.695	2.583	3.041	4.003	5.336
	DiD_IPT	1000	0.000	1.026	-3.045	-2.309	-1.748	-1.392	-0.695	0.030	0.744	1.328	1.628	2.208	2.706
Negative intraperiod return	*ΔIPT(control)*	1000	0.223	1.457	-3.229	-2.686	-2.137	-1.725	-0.878	0.171	1.293	2.236	2.641	3.106	3.785
	ΔIPT(treat)	1000	0.276	1.350	-2.855	-2.348	-1.820	-1.481	-0.781	0.208	1.308	2.111	2.459	3.155	3.573
	DiD_IPT	1000	-0.053	0.495	-1.520	-1.129	-0.853	-0.723	-0.414	-0.048	0.312	0.608	0.726	1.003	1.260

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TABLE E.1 - *Continued*

This table presents the null distributions of the portfolio-level *ΔIPT* and *DID_IPT* created under the null hypothesis that the order of the monthly returns does not matter. Panels A and B present the distributions for the unmatched and matched H2 samples (table 3.4, panel B), respectively, and correspond to the results in table 4.18. The matched sample is identified using CEM on *OCFVOL* (28 cutpoints), *MVE* (8 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *ΔIPT(control)* is the post-period *IPT* minus the pre-period *IPT,* within the control group. *ΔIPT(treat)* is the post-period *IPT* minus the pre-period *IPT,* within the treatment group. *DiD_IPT* is equal to *ΔIPT(treat)* minus *ΔIPT(control).* All variables are defined in Appendix A.

TABLE E.2

Null Distribution of IPT - Constant Derivative Sample (H2)

Panel A: Constant Derivative Unmatched Sample

Sample/Subsample	Statistic	N	mean	sd	min	p1	p5	p10	p25	p50	p75	p90	p95	p99	max
Combined	*ΔIPT(control)*	1000	0.415	1.036	-2.597	-2.008	-1.347	-0.950	-0.306	0.430	1.157	1.804	2.070	2.639	3.075
	ΔIPT(treatment)	1000	0.481	0.798	-1.765	-1.335	-0.837	-0.555	-0.082	0.480	1.037	1.540	1.788	2.302	2.553
	DiD	1000	0.067	0.538	-1.299	-1.066	-0.776	-0.621	-0.310	0.035	0.416	0.792	0.992	1.331	1.555
Positive intraperiod return	*ΔIPT(control)*	1000	0.711	1.238	-2.672	-2.199	-1.392	-0.917	-0.137	0.748	1.587	2.360	2.688	3.358	3.779
	ΔIPT(treatment)	1000	0.751	1.072	-1.778	-1.423	-0.955	-0.674	-0.057	0.736	1.551	2.228	2.531	3.067	3.553
	DiD	1000	0.040	1.167	-3.193	-2.322	-1.840	-1.488	-0.804	0.052	0.801	1.608	2.041	2.567	3.205
Negative intraperiod return	*ΔIPT(control)*	1000	0.351	1.082	-2.433	-1.941	-1.341	-1.029	-0.504	0.327	1.173	1.784	2.196	2.668	3.084
	ΔIPT(treatment)	1000	0.272	1.667	-3.819	-3.185	-2.428	-1.868	-1.028	0.291	1.509	2.459	2.950	3.898	4.366
	DiD	1000	-0.079	0.906	-2.699	-1.998	-1.581	-1.255	-0.722	-0.084	0.566	1.126	1.416	1.954	2.381

Panel B: Constant Derivative Matched Sample

Sample/Subsample	Statistic	N	mean	sd	min	p1	p5	p10	p25	p50	p75	p90	p95	p99	max
Combined	*ΔIPT(control)*	1000	0.423	1.357	-3.240	-2.641	-1.724	-1.333	-0.594	0.428	1.420	2.160	2.573	3.516	4.217
	ΔIPT(treatment)	1000	0.482	1.016	-1.966	-1.712	-1.174	-0.836	-0.237	0.467	1.194	1.801	2.170	2.929	3.768
	DiD	1000	0.058	1.039	-2.994	-2.324	-1.654	-1.257	-0.680	0.091	0.779	1.451	1.808	2.265	2.874
Positive intraperiod return	*ΔIPT(control)*	1000	0.604	1.457	-3.179	-2.733	-1.775	-1.299	-0.410	0.595	1.641	2.396	2.945	3.984	5.127
	ΔIPT(treatment)	1000	0.672	1.416	-2.697	-2.240	-1.634	-1.175	-0.333	0.650	1.671	2.582	2.992	3.987	5.045
	DiD	1000	0.068	1.366	-3.694	-3.008	-2.175	-1.723	-0.906	0.041	1.045	1.795	2.412	3.130	3.797
Negative intraperiod return	*ΔIPT(control)*	1000	0.308	1.485	-3.571	-2.784	-2.119	-1.585	-0.785	0.312	1.413	2.350	2.762	3.369	3.973
	ΔIPT(treatment)	1000	0.255	1.482	-3.543	-3.156	-2.083	-1.684	-0.802	0.257	1.327	2.170	2.708	3.445	4.425
	DiD	1000	-0.053	0.791	-2.111	-1.795	-1.313	-1.078	-0.597	-0.052	0.511	0.984	1.256	1.707	2.185

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This table presents the null distributions of the portfolio-level *ΔIPT* and *DID_IPT* created under the null hypothesis that the order of the monthly returns does not matter. Panels A and B present the distributions for the constant derivative unmatched and matched H2 samples (table 4.19), respectively and correspond to the results in table 4.20. The constant derivative samples exclude any firm-years whose derivative use (non-use) in the post-period is inconsistent with classification in the pre-period. The matched sample is identified using CEM on *OCFVOL* (26 cutpoints), *MVE* (10 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *ΔIPT(control)* is the post-period *IPT* minus the pre-period *IPT*, within the control group. *ΔIPT(treat)* is the post-period *IPT* minus the pre-period *IPT*, within the treatment group. *DiD_IPT* is equal to *ΔIPT(treat)* minus *ΔIPT(control)*. All variables are defined in Appendix A.

TABLE E.3

Null Distribution of IPT - Alternative Matched Samples (H2)

Panel A: Constant Derivative *OCFVOL* -Matched Sample

Sample/Subsample	Statistic	N	mean	sd	min	p1	p5	p10	p25	p50	p75	p90	p95	p99	max
Combined	*ΔIPT(control)*	1000	0.348	1.078	-2.612	-1.977	-1.421	-1.108	-0.418	0.393	1.110	1.759	2.074	2.665	3.342
	ΔIPT(treatment)	1000	0.455	0.974	-2.023	-1.702	-1.185	-0.811	-0.230	0.452	1.155	1.751	2.071	2.518	3.247
	DiD	1000	0.107	0.476	-1.305	-0.952	-0.691	-0.511	-0.242	0.115	0.456	0.737	0.880	1.115	1.361
Positive intraperiod return	*ΔIPT(control)*	1000	0.573	1.153	-2.652	-1.940	-1.300	-0.955	-0.259	0.632	1.387	2.061	2.454	3.198	4.106
	ΔIPT(treatment)	1000	0.698	1.143	-2.052	-1.584	-1.142	-0.834	-0.145	0.671	1.522	2.238	2.551	3.373	3.861
	DiD	1000	0.125	0.828	-2.362	-1.724	-1.200	-0.930	-0.474	0.132	0.724	1.158	1.501	1.937	2.421
Negative intraperiod return	*ΔIPT(control)*	1000	0.300	1.431	-3.334	-2.739	-1.971	-1.551	-0.753	0.255	1.399	2.192	2.601	3.355	3.788
	ΔIPT(treatment)	1000	0.172	1.581	-3.742	-3.120	-2.349	-1.892	-1.065	0.125	1.399	2.327	2.754	3.390	3.950
	DiD	1000	-0.128	0.732	-2.148	-1.844	-1.385	-1.110	-0.641	-0.100	0.369	0.833	1.057	1.429	1.642

Panel B: Constant Derivative *MVE* -Matched Sample

Sample/Subsample	Statistic	N	mean	sd	min	p1	p5	p10	p25	p50	p75	p90	p95	p99	max
Combined	*ΔIPT(control)*	1000	0.446	1.213	-2.496	-2.154	-1.593	-1.042	-0.387	0.372	1.322	2.088	2.517	3.131	3.551
	ΔIPT(treatment)	1000	0.466	0.905	-1.893	-1.573	-0.957	-0.677	-0.185	0.414	1.049	1.732	2.081	2.568	3.106
	DiD	1000	0.019	0.76	-2.124	-1.759	-1.213	-0.974	-0.505	0.012	0.556	1.008	1.258	1.657	1.951
Positive intraperiod return	*ΔIPT(control)*	1000	0.646	1.458	-2.76	-2.382	-1.722	-1.148	-0.336	0.502	1.707	2.659	3.162	4.046	4.757
	ΔIPT(treatment)	1000	0.714	1.234	-2.334	-1.812	-1.167	-0.773	-0.213	0.658	1.521	2.389	2.876	3.848	4.731
	DiD	1000	0.069	1.139	-3.235	-2.521	-1.833	-1.407	-0.712	0.065	0.894	1.542	1.957	2.524	3.116
Negative intraperiod return	*ΔIPT(control)*	1000	0.233	1.193	-2.823	-2.239	-1.661	-1.29	-0.661	0.168	1.186	1.858	2.209	2.726	3.121
	ΔIPT(treatment)	1000	0.269	1.37	-2.981	-2.525	-1.928	-1.49	-0.787	0.202	1.298	2.176	2.612	3.202	3.838
	DiD	1000	0.037	0.583	-1.393	-1.251	-0.908	-0.7	-0.373	0.02	0.438	0.794	1.017	1.38	1.921

Continued on next page

TABLE E.3 - *Continued*

Panel C: Constant Derivative *OCFVOL*-*ANALYSTS_N*-Matched Sample

Sample/Subsample	Statistic	N	mean	sd	min	p1	p5	p10	p25	p50	p75	p90	p95	p99	max
Combined	*ΔIPT(control)*	1000	0.444	1.071	-2.314	-1.857	-1.236	-0.971	-0.381	0.455	1.249	1.865	2.167	2.759	3.235
	ΔIPT(treatment)	1000	0.458	0.840	-1.846	-1.374	-0.946	-0.657	-0.138	0.455	1.051	1.574	1.842	2.356	2.794
	DiD	1000	0.014	0.728	-2.076	-1.592	-1.167	-0.949	-0.519	0.028	0.547	0.958	1.211	1.577	1.788
Positive intraperiod return	*ΔIPT(control)*	1000	0.632	1.119	-2.193	-1.804	-1.134	-0.819	-0.179	0.609	1.446	2.097	2.566	3.115	3.724
	ΔIPT(treatment)	1000	0.673	1.096	-2.207	-1.599	-1.030	-0.694	-0.115	0.561	1.412	2.179	2.659	3.245	4.037
	DiD	1000	0.041	0.964	-2.769	-2.088	-1.493	-1.214	-0.644	0.007	0.767	1.320	1.694	2.109	2.697
Negative intraperiod return	*ΔIPT(control)*	1000	0.216	1.551	-3.862	-2.987	-2.272	-1.799	-0.972	0.238	1.332	2.387	2.748	3.471	4.259
	ΔIPT(treatment)	1000	0.227	1.579	-4.219	-3.235	-2.315	-1.756	-0.949	0.205	1.408	2.339	2.761	3.513	4.355
	DiD	1000	0.011	0.680	-1.939	-1.520	-1.113	-0.883	-0.489	0.026	0.488	0.907	1.140	1.453	1.652

This table presents the null distributions of the portfolio-level *ΔIPT* and *DID_IPT* created under the null hypothesis that the order of the monthly returns does not matter. Panels A, B and C present the distributions for the constant derivative *OCFVOL*-matched, *MVE*-matched, and *ANALYSTS_N-MVE-* matched H2 samples, respectively and correspond to the results in table 4.21. The constant derivative samples exclude any firm-years whose derivative use (non-use) in the post-period is inconsistent with classification in the pre-period. The constant derivative *OCFVOL*-matched sample is identified using CEM on *OCFVOL* (44 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. The constant derivative *MVE*-matched sample is identified using CEM on *MVE* (10 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. The constant derivative *OCFVOL-ANALYSTS_N*-matched is identified using CEM on *OCFVOL* (33 cutpoints), *ANALYSTS_N* (5 cutpoints), Fama-French 12 industry classifications and the sign of intraperiod return, within the constant derivative unmatched sample. Treatment firms include derivative users and control firms include derivative non-users, identified in the pre-SFAS 133 period. The pre-period includes fiscal years ending June 1999 to May 2000 and the post-period includes fiscal years ending June 2001 to May 2002, to correspond with the effective date of SFAS 133. Positive (negative) intraperiod return subsamples include observations whose 12-month buy-and-hold abnormal returns are positive (negative) in both the pre- and post-periods. *ΔIPT(control)* is the post-period *IPT* minus the pre-period *IPT*, within the control group. *ΔIPT(treat)* is the post-period *IPT* minus the pre-period *IPT,* within the treatment group. *DiD_IPT* is equal to *ΔIPT(treat)* minus *ΔIPT(control)*. All variables are defined in Appendix A.